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## ORIGINAL ARTICLES.

### I.—Professor J. W. Spencer on Changes of Level in Mexico.

By Prof. Edward Hull, M.A., LL.D., F.R.S., F.G.S.

THROUGH the courtesy of Professor J. W. Spencer, I have received an early copy of his paper on the "Great Changes of Level in Mexico and the Interoceanic Connections,"[1] containing the observations and conclusions derived from a visit paid to this region in 1895, with the special object of determining on the spot whether the suggestion that the drainage of what is now the Gulf of Mexico formerly crossed the Tehuantepec Isthmus into the Pacific. This paper is not less interesting and important than the previous memoir by the same author on the "Reconstruction of the Antillean Continent," a résumé of which has appeared in the Geological Magazine (April, 1895) by the pen of Mr. A. J. Jukes-Browne; but in this latter case Professor Spencer was dealing mainly with facts and inferences based on physical features under submerged areas of the Atlantic Ocean, while in the present case he has to deal with phenomena visible to the eye of the observer.

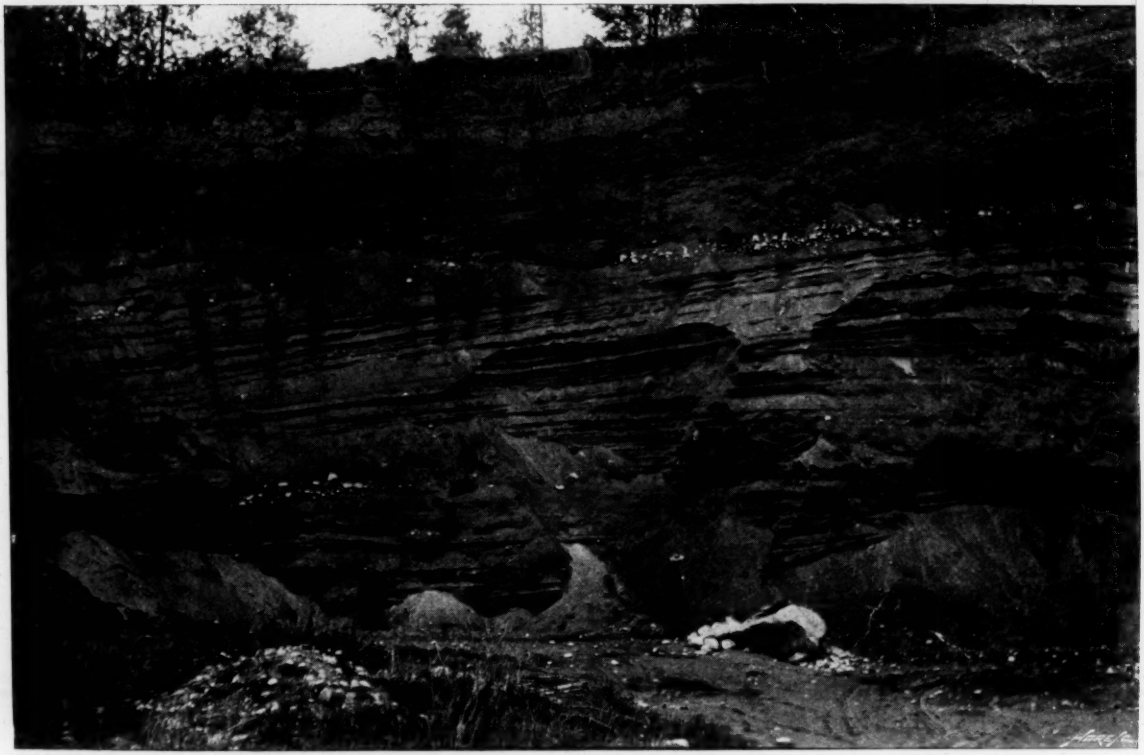
It will be recollected that in expounding the hypothesis of a former Antillean Continent the idea of a submergence of the Mexican region and the Isthmus of Tehuantepec, sufficient to allow of interoceanic communication, was in the author's mind a necessary corollary. The "drowned rivers" and their present affluents, such as the Mississippi, being shut out from the Atlantic, required an outflow in the opposite direction into the Pacific. The position of this outflow Professor Spencer has now determined with what appears absolute certainty in the "divide" of the Tehuantepec Isthmus at levels of about 1,000 feet above the ocean; but before describing this channel of interoceanic communication in more detail, some account must be given of the Mexican topography, as very clearly described and illustrated by photographs and drawings by Professor Spencer.

The region presents the aspect of a series of steps or plateaux, from the most elevated at a height of 10,000 to 11,000 feet above the ocean down to the coastal plains, forming, in the author's view, "base-levels of erosion," and breaking off in escarpments which are intersected by ravines or cañons, cut back by the rivers to greater or less distances into the plateaux themselves. The levels differ

[1] Bull. Geol. Soc. Amer., vol. ix, pp. 13–34.

somewhat in height in the Mexican plains and the isthmus, sloping downwards from the continental tracts towards the Tehuantepec Isthmus. Thus the coastal plains of Mexico (which are continuous with those of the United States) descend from a level of 1,600 to 1,700 feet to 325 feet in the isthmus, where they abut against the base of the high plateaux. It is inferred that these plains represent pauses in the emergence or subsequent submergence of the region, and represent on the land the features represented by the "drowned plains and river-valleys" of the Atlantic coast, the West Indian Islands, and the Mexican Gulf. They are generally covered by loam, gravel of rounded pebbles, or calcareous marl with shells; sometimes of lacustrine, sometimes marine, origin. Most important amongst the Tertiary deposits is the "Coatzacoalcos formation" of the Tehuantepec Isthmus on the northern side, and so called from the river of that name which traverses the formation. It consists of calcareous clay, of uniform texture and horizontal stratification, containing a large number of marine forms collected by the author and determined by Dr. W. H. Dall. Of the fossils found 34 per cent. are not known to be living forms, so that they may be regarded as of late Miocene or early Pliocene age; but as the characters of the living fauna of the deeper waters of the Gulf of Mexico are only partly known, Dr. Dall suggests that the fossils collected represent a much larger percentage of living forms than those named in the list given in the memoir of the author. It does not appear to what altitude this formation extends, but it has suggested to Professor Spencer that there may have been many Pliocene connections between the Gulf and Pacific Ocean throughout a distance of 200 miles during the greatest submergence. Inland from Vera Cruz a belt of Tertiary limestone and marl has been mapped by the Geological Survey of Mexico, reaching levels of 2,350 to 2,800 feet above the sea, which may not improbably be representative of the Coatzacoalcos formation.

The "Lafayette" and "Columbian" formations, each unconformable to the other, are found occupying eroded basins in these middle Tertiary strata, rising to high altitudes, and indicating deep Mid-Pleistocene depression, on the supposition that they represent deposits formed along the sea-margins of the period. In any case, however, the inference of a deep submergence during the Pliocene stage seems inevitable. According to the author, the Lafayette formation seems to have succeeded the Coatzacoalcos in the Tehuantepec region without any considerable physical disturbance intervening; but on the Pacific side the mechanical materials were replaced by a white soft limestone with water-worn pebbles. It would be of interest to know whether this limestone contained marine forms, and if so, whether of existing species. We must now refer to the position and nature of the divide (or neck of land) which at a late Pleistocene epoch formed the floor of the connection between the Gulf of Mexico and the Pacific. The details given by the author are illustrated by photographic views. The Tehuantepec highland is at this spot reduced to the narrow width of only 25 miles, bounded



The Great Ås at Upsala, Sweden.

by two coastal plains. The high plateau of Mexico, with levels up to 10,000 feet, here descends for a distance of 60 to 80 miles, to levels of 2,000 to 4,000 feet, and at the divide is (as already stated) only about 1,000 feet above the level of the ocean; but on both sides of the saddle there are base-levels of lower altitudes. The rock consists of earthy sandstone; and on the Pacific slope facing the city of Tehuantepec are old sea-cliffs and caves at levels of 400 feet. The floor of the divide is traversed by a "geological canal," at a level of 776 feet above the ocean, and is covered by gravel, from 4 to 8 feet in depth, of quartz and soft sandstone pebbles, the latter being well rounded; this gravel is more thinly scattered over the adjoining slopes to a height of 150 feet. The isthmus was evidently swept over by ocean-currents passing through the straits during submergence; and its elevation has been so recent that only short cañons have been cut into the base-levels and terraced plains adjoining. Another line of communication was recognized at the pass of Tarifa, a dozen miles eastward of that of Chivela above described; and there are other current-swept depressions in this region through which the waters on both sides are considered to have had intercommunication, though at higher levels. It should be added that the stratified gravel of the divide is continuous with that covering the terraced plains on the Gulf side of the divide.

The author considers that this oceanic connection was as old as the Columbian (Mid-Pleistocene) epoch, and was contemporaneous with the great emergence of the Antillean Continent and Eastern America. It is a splendid illustration of the Lyellian doctrine of the interchange of land and sea, which geological phenomena bear testimony to from early, down to recent, times, and which serves as a key to many problems in terrestrial physics. Finally, it must not be forgotten that the biological evidence of the former oceanic communication across the Isthmus of Panama is not less clear than is the physical. The late Dr. W. B. Carpenter identified 35 species of molluscs, out of 1,400 Pacific forms, as occurring on the Atlantic side of this region; the number having been since increased to 100 species, by the observation of Mr. Charles T. Simpson; while, according to the late Dr. G. B. Goode, there is absolutely no resemblance between the deep-water fishes on the two sides of Central America.

## II.—The Surface Geology of the North of Europe, as illustrated by the Åsar or Ósar of Scandinavia and Finland.

By Sir Henry H. Howorth, K.C.I.E., M.P., F.R.S., F.G.S.

PLATE VII.

In a previous paper I ventured to emphasize the opinion, now very generally held, that, whether by a gradual process or spasmodically, the Northern and Central parts of Scandinavia have been rising from the sea-level since Tertiary times, and that, so far as we know, this rise has not been interrupted by intervals when the movement has been one of depression. The movement has, in fact,

The Great Ås at Upsala, Sweden.

been constantly in one direction. If this be true, it follows as certainly as any physical fact follows its efficient cause that, other conditions being the same, the climate of Scandinavia, like that of Greenland, has been continually growing more severe, and is more severe now, than it was when the higher Norwegian raised beaches were deposited.

The other conditions, however, have, so far as we can judge, not been uniform. One of them, and that a very important one, has in all probability altered, and that is the one which gives Norway and Britain their exceptional climate, and which diverts the isothermal lines of Western Europe from their normal route across Asia and America in places on the same latitude. This is the Gulf Stream. There are very strong reasons (and I have formulated some of them in my "Glacial Nightmare") for believing that at the close of the Tertiary period the so-called Gut of Florida was blocked by solid land, and in consequence the warm waters of the Gulf of Mexico did not then get into the North Atlantic. If the Gulf Stream were non-existent, it is clear that the climate of the two sides of the Atlantic would be more alike than they are now along the same latitudes. That this was so is proved by the more Arctic types of molluscs which then lived on the coasts of Scandinavia and Scotland, and by the evidence of the existence of Alpine and Arctic plants at lower levels and in lower latitudes in Western Europe, as shown by Nathorst and C. Reid. This conclusion is not only reasonable, but seems incontrovertible. It does not mean that North-Western Europe was then dominated by a Glacial climate and Glacial conditions, but only that it was more or less assimilated in regard to its climate to Canada and New England.

As we have also seen, the evidence is very strong and conclusive, and has convinced almost every Swedish geologist, that not only has the greater part of Scandinavia and Finland risen greatly in altitude in the last geological period, but that this wide area has in a large measure been actually submerged under the sea since Tertiary times, and that its rise after this submergence was the last great fact which affected its surface.

I have argued that it was this submergence which did so much to polish and mammillate its rock-surfaces, effects which I hold to be the results in a very large measure of the eroding forces of the sea in a tempestuous latitude, and not of the hypothetical ice-sheet of which we have read so much. I will add another argument to those already used. If the terraces on the Norwegian coast really mark, as the Norwegian geologists argue, the differential rate of elevation of the coast, which has caused the cutting back of the cliffs to be more rapid at one time than at another, it is clear that the *polished rock faces* of Norway cannot be due to anything but the corroding sea, for these faces have been worn back many feet while the coast has been rising, and cannot therefore retain any polish or smoothness they may have acquired in the times preceding the upheaval. If they were polished by ice, the ice must have acted, not before, but after the elevation, which is a *reductio ad absurdum*. Apart from

this, the facts presented by the general contour and face of the country seem to me to inevitably point the same lesson. Whether we examine the string of islands which fringe so much of the coast of Scandinavia, and which project from the surrounding water like so many gigantic whales' or porpoises' or turtles' backs; or whether we examine the thousand islands of the Mälar Sea or the Äland Archipelago, with the same contours, or the mammillated surfaces which the gneissic and granitic rocks of the interior districts of Sweden and Finland bear, they seem to me to present a complete parallel to the contour of the islands of the Arctic archipelago north of America, and of the islands off the coast of Greenland, where the lines of drift wood and the stranded whales far above high-water most conclusively point to the whole land having recently risen from the sea. In these latter cases, the Arctic navigators who have seen the phenomena, and the geologists who have described their voyages, have agreed that the North American archipelago and the islands off Greenland have had their contours smoothed and rounded by that most effective of denuding agencies, a shallow ocean loaded with gravel and other débris, and not by an ice-sheet, which does not in fact exist there. Nor, to take another illustration, can we separate in any way, it seems to me, at Trollhättan and elsewhere the polishing and smoothing of the interior and of the lips of the Giants' Cauldrons, which are confessedly the result of the aqueous action just named, from the polishing of the inclined rock-surfaces on which they occur. There is absolute continuity between them. They all seem to me to concur with the upraised shell-beds, the great masses of false-bedded and stratified sands on the wide upland plains of Dalecarlia, and the other evidences which have been collected by the Swedish geologists, and to which I have referred in a previous paper, to show, not that the country has been swathed in ice, but that it has recently been the bed of a shallow and tempestuous sea. This conclusion is of the highest importance. It is not, of course, new. Without going back to the primitive geologists of the early part of the last century, who wrote before the incubation of the Glacial monster, it struck some of the very earliest critics of that theory, who had examined the problem very thoroughly on the ground itself.

Böhtlingk, an experienced observer and a great traveller in Lapmark and Finmark, to whom we owe the conclusive evidence against polar ice-caps, says: "In Scandinavia, Finland, Lapland, and the surrounding countries we find, to the height of 800 feet, the most unquestionable marks of the constant retreat of the sea occasioned by a continued rise of the land. In consequence of this circumstance Scandinavia, during the first half of the alluvial period, was still an island, and the tongues of land of Russian Lapland, Finland, Esthonia, the government of Olonetz, as well as those parts of the government of Archangel situated to the east of the White Sea, were covered by the sea," etc. (Ed. Journ., vol. xxxi, 1841, pp. 354, 355.)

Robert, the very able geologist of the Recherche Expedition, writing as far back as 1843, says: "La mer me semblait polir,

canèler, creuser, rayer des roches de manière à leur faire prendre la physionomie de ceux qui s'offrent aujourd'hui un peu au dessus de son niveau sur toutes les côtes de la Scandinavie." Elsewhere he concludes that the sea once occupied a large part of Russia, and that Scandinavia then formed an archipelago. I have myself zigzagged across Sweden in various directions on my recent third visit to that country, and been continually impressed by the same conclusion.

The most powerful and important evidence has yet to be quoted, however, and it is forthcoming from what every intelligent person, who has traversed Sweden with the view of studying its recent geology, must consider to be in their way the most interesting and stupendous phenomena probably in the world: I mean the Swedish åsar or ôsar. I am writing this paper in the very midst of them, and have had some special opportunities of examining them. The latest writer on Swedish geology, and one of the ablest, Nathorst, in his "Sveriges Geologi," published in 1894, after examining the various theories which have been forthcoming to explain them, has to confess that the problem is still unsolved. To use his own words, "vilja vi dock på samma gång uttryckligen betona, att vi ännu icke betrakta frågan såsom slutligen afgjord" (op. cit., p. 243)—"we must expressly state that we cannot consider (or look upon) the question as finally settled."

The åsar are such a notable feature in the landscape of Sweden that it is not surprising they should have been observed and their peculiarities described at an early period. Their main features were, in fact, pointed out by Swedenborg at the beginning of the last century, and have been enlarged upon by every succeeding explorer. The Swedish geologists divide the åsar into two classes—the åsar properly so called, built up of masses of rolled stones, and the sand-åsar, composed chiefly of sand. While it is easy to find specimens of each of these, it is also very easy to find others where masses of rolled stones and beds of sand or of tough clay or brick-earth pass into each other very much as they do in the Cromer cliffs. A good example is the fine ås upon which Upsala is built, and in which we can study the internal structure admirably, since it has been recently excavated right through (vide Pl. VII). There we can see in the course of a few yards the passage from a mass of rounded boulders into sand. The sand in some places is almost continuous, and in others has banks of clay intercalated in it. The contour of the åsar, as Swedenborg long ago pointed out, differs with the nature of their contents, the stony åsar having steep sides, while the sandy ones have much rounder outlines. The stones which form such a great part of the åsar (except certain specimens occurring in their upper parts) are invariably rounded and water-worn, and would be well described by the phrase applied to some of the East Anglian gravels, viz. "cannon-shot gravel." The åsar are found in all parts of Sweden from Scania to Norland, and in Finland and Northern Russia they form, as is well known, huge banks and ramparts. In some cases they run with great uniformity in shape and breadth for long distances, their direction being

wonderfully continuous. So uniform are they that, as Brongniart pointed out, the roads in some places, as from Upsala to Wendel, from Enköping to Nora, from Hubbo to Moklinta, etc., run along their crest. Sometimes they spread and widen out a little, forming nodes like so many knots on a cord. Frequently the continuous line is interrupted by a gap or a series of gaps, so that instead of a uniform bank there are a number of huge circular or oval mounds. They consist generally of a main trunk, with a number of small subsidiary lateral branches running into them like the affluents of a river, and sometimes they have satellites attached to them in the shape of eskers and kame-like mounds. They are as sharply marked off from the adjoining plain on either side as a railway embankment is. In some cases, notably in Finland, they do not run in parallel lines, but vary in direction, sometimes even crossing each other, but in Sweden their direction is singularly parallel, as may be seen from the admirable maps published by the Swedish geologists, notably that by Törnebohm. The enormous size and cubical contents of these gigantic mounds can only be appreciated by those who have seen them on the spot and followed them for miles.

According to Erdmann, the well-known Upsala ås, which runs from the mouth of the Dalelf to Södertom, south of Stockholm, is about 200 kilometres long. The ås of Koping, as far as it is at present traced, from Nyköping to the Dalelf, is about 240 kilometres in length. The ås of Enköping runs from near Trosa in Sudermannia to Loos in Helsingland, and is from 300 to 340 kilometres long, while the ås of Badelunda, running from Nyköping in Sudermannia to the parish of Rättvik in Dalecarlia, is about 300 kilometres long. According to Erdmann, the åsar west of the watershed between Lake Wenern and Lake Wettern run N.N.E.–S.S.W., while east of that line they run from N.N.W. to S.S.E.

Erdmann also gives the elevation at which some of the principal åsar have been traced. "In Jemteland, N. and N.W. of Storojo, to 1,000 or 1,200 feet; in Herjeadal, near Hede, to 1,300 or 1,400 feet; in Dalecarlia, in the parishes of Malung and Idre, to between 1,000 and 1,300 feet; in the government of Elfsborg, in Vestrogothland and east of Ulricehamm, to 1,100 feet; at Jönköping, in Småland, near to Lake Almesäkra, to about 1,000 feet; but Törnebohm informed Mr. Geikie that in the northern parts of the country they occur at an elevation of 2,000 feet." Their height varies, the average being about 50 or 100 feet high, but in many places they run up to 100 metres or more, while they sometimes sink to 20 or 30 feet. Their breadth, too, varies, the normal breadth being from 30 to 50 paces, but in some cases, as at Upsala, where there is a spreading node, their breadth runs to 200 or 250 yards. From these facts the cubical contents of the åsar may be guessed. They are often somewhat wider and higher at their northern end, that is, at their inception, than further on. In the low flat country their contour is very uniform, but in the upper and more hilly districts, where they chiefly abound, they have a tendency to become broken up into strings of separate mounds and kame-like masses. Their materials,

in so far as they consist of boulders, have in every case where they have travelled, and we can trace the mother rock *in sitû*, moved from north to south, and were never in the reverse direction.

One of their most important features, and one which has been a great deal too little noticed in the various theories which have been forthcoming to explain them, is the fact that they traverse the country quite irrespective of its contour, going uphill and downhill, and athwart the natural drainage. On this point I will quote the language of a first-rate authority, Erdmann. After saying that they sometimes run along the valleys, sometimes on the mountain flanks, and sometimes on the plateaux, he adds (in italics) the words: "C'est ainsi qu'elles continuent leur cours lointain, franchissant les plateaux, les vallées, et les plaines, et ne semblant en aucune manière s'inquiéter des reliefs divers actuels du pays." ("Exposé," etc., p. 41.) This is a conclusion drawn from the Swedish åsar. The Finnish ones are quite as remarkable, traversing lakes and watersheds without any hesitation.

As I have said, a large portion of the åsar consist of masses of rounded stones of various sizes up to 2 feet in diameter. These rounded stones are not mixed with angular erratics. The latter, when they occur, do so in the upper and more sandy and loamy layers, or scattered over the åsar's backs, nor, so far as I could observe, do they contain stones of exceptional size. These, again, chiefly occur in the sandy beds or on the backs of the åsar. Their contents are not sorted according to their size, but the stones generally lie with their longer axes parallel to the direction of the ås in which they are found. The beds of sand and the sandy åsar are in nearly all cases more or less stratified. They are frequently false-bedded, and the beds which show the false-bedding have their lines very pronounced, the angular wedges of sand and the lenticular masses being on a large scale and very marked. The uppermost layers of the åsar often consist of stiff blue clay or of finely sifted and laminated brickearths, containing in places numbers of diatoms and marine shells, but never, so far as I know, fresh-water débris or land molluscs. These beds of brickearth and clay occur only at the top of the åsar, where they are often intercalated with sand beds very irregularly disposed, just as they are in the beds of contorted drift in the Cromer cliffs, and they are generally continuous with the mantle of similar loam that covers the intervening country. I cannot follow Erdmann and Geikie in separating these superficial layers in the åsar from the beds below. So far as I can judge (and here, again, the present condition of the cutting at Upsala is very pregnant with meaning), they pass continuously down into them, and are merely later phases of one deposit, just like the similar phases we see in the drift beds of East Anglia. Lyell, Murchison, and others, who examined the åsar with care and skill, and whose judgment was in this case unwarped by *a priori* theories of the origin of the åsar, treated the superficial beds containing marine shells as belonging to the same period as the lower beds, which are barren and consist largely of boulders.

Let us now consider the theories which have been adopted to explain the åsar. In the very early days of the Glacial fever—if I may coin an incongruous but not inappropriate phrase—when Agassiz reigned supreme, they were pronounced to be moraines. This conclusion is one of those which form the despair of rational science, for beyond the fact that they are heaped-up mounds of earth and stones, there does not seem to be a single feature about them resembling moraines. The stones they contain are rounded, water-worn boulders, in no way like glacier stones. Scratched stones, or those with flat sides, are never found in them. The beds of sand and clay they contain are sifted out and separated from the boulders, and are stratified and absolutely different to the mixed-up heterogeneous "muck" forming moraine stuff. The shells they contain in their upper layers are marine shells, many of them perfect and of very delicate texture. Marine shells and diatoms are not the product of ice-sheets or of glaciers, and do not occur in moraines. Putting their contents aside, their other features are quite different to moraines. Terminal moraines, which are the only kind of moraines distinctly resembling some phases of the åsar in contour, are always planted athwart the line of march of the ice. The åsar, on the contrary, are all roughly parallel to the line in which the stones have moved, and to the line also kept by the striæ on the rocks. If moraines at all, the åsar must therefore be medial or lateral moraines. Who has ever seen lateral or medial moraines made up of water-worn boulders and of stratified sands and brickearths containing marine shells, or seen them ranged in a large series of parallel mounds with subsidiary branches, and with no high lands in between from which their contents could be derived? But I need not press the argument further.

Berzelius, in a letter to Professor Leonhard written as far back as 1841, says: "Agassiz' friend Desor visited us in September last year, and on seeing the immense boulder deposits which in this country are named åsar, stated without hesitation that these phenomena could not be explained by glaciers, and that they were not moraines." (Q.J.G.S., iii, 76.)

Durocher also long ago analyzed the various features of the åsar in a masterly manner, comparing them point by point with moraines and their structure, and showed how completely they differed from them. Reclus, who, although not a professed geologist, has treated geological problems with great intelligence in his great geographical work, is not less emphatic in his conclusion. Murchison and Verneuil and other "old masters" who examined the problem on the ground were of the same opinion. Nor do I know of any Scandinavian geologist who now maintains the view that the åsar are moraines. If there be any geologists that do so anywhere, it must be in America, where the most extravagant school of glacialists survives, and where official geology is so dominant, and every officer of the Survey is apparently so dragooned by the conditions of the service, that they follow their bellwethers with commendable loyalty and discipline.

If not moraines, what are the åsar? Hisinger suggested that they might be the remains of a gigantic denudation, the intervening deposits having been swept away. This view, while it did not in any way explain the internal structure of the åsar, merely professed to explain their external shape and distribution. It has been completely analyzed by Törnebohm, and shown to be quite untenable; nor do I know anyone who now holds it, or who in fact professes to understand how such a denudation could come about. What kind of diurnal or other denuding agency would permit of these ramparts of soft materials remaining as they are when the rest of the beds were swept out? Whence could it come? How could it work so as to move up and down the country irrespective of its contour? Where has the débris of the gigantic denuding process gone to? How is it that the covering of the åsar, which is formed of finely levigated brickearths, is also the covering of the intervening plains on either side? But I will not argue against a cause which has no defenders, nor kill again the corpses which Törnebohm slew.

Every Scandinavian geologist known to me now admits that the åsar are in some way the result of aqueous action. The contour of their surface, the rounding and arrangement of the boulders in them, with their longer axes symmetrically placed parallel to the lines of the ramparts, the stratified sands and laminated clays, the current bedding, the presence of shells and diatoms, are all conclusive that the åsar are the result of aqueous action in some form or other; and Mr. James Geikie himself, who represents the high-water level of English and Scotch glacialism, says "all geologists admit that the åsar are in the main water-formed accumulations." Erdmann, Törnebohm, Nathorst, and all the other Northern geologists known to me, are of the same opinion. When we come, however, to discuss the particular kind of aqueous agency to which the åsar may be assigned, and the method in which it worked, the unanimity at once ceases.

The superficial resemblance of the åsar, when drawn on a sheet of paper, to rivers with a main trunk and branching off into smaller affluents, perhaps first suggested the idea that they had something to do with rivers and river action; a view which has prevailed very considerably in textbooks, but which seems to me to be absolutely untenable.

Two theories of the fluviatile origin of the åsar have been propounded, one treating them as the result of subaerial rivers and the other as subglacial streams. I would first criticize the general theory of fluviatile origin.

In the first place, as we have seen, the åsar do not run along level surfaces nor along continuous slopes; but they frequently run up and down hill. Sometimes they are found at a height of 2,000 feet and sometimes only a few feet above the sea-level, and they run up and down the undulating country keeping the same general direction. Now whatever movements are possible with ice under certain conditions, by which it may

be able to move up and down slightly undulating districts, and sometimes to creep uphill to a moderate extent, it would be an entirely new and surprising fact that water could do so, unless contained in a pipe and forced up by pressure behind. This is an initial difficulty of the first moment, and is in fact absolutely conclusive. Water, except in a pipe, cannot move contrary to gravity, cannot travel up and down hill, or mount a slope; and it does not matter whether the water is in a channel open to the sky, or in a channel covered with an arched tunnel of ice. It is therefore impossible on this ground alone that the åsar could have been deposited by rivers of any kind, unless the contour of the country has entirely and radically changed since they were laid down.

This is by no means the only objection to the fluviatile theory of the åsar. Their shape, when viewed in section, is quite opposed to a fluviatile origin. Rivers which run very slowly and carry much mud, instead of depositing that mud entirely in deltas, sometimes, no doubt, raise their own beds, like the lower Rhine and some rivers of Eastern England do, and in this way make themselves solid aqueducts along which they flow. These solid aqueducts, however, have not the shape or contour of åsar, with their often steep and sharply inclined sides. This contrast in contour is even more marked in the heaps of débris which form the beds of subglacial streams. Nor can I see how rivers of any kind could raise their beds to the portentous height of the åsar and yet be so narrow. Rivers, again, must have banks, and if of fluviatile origin the åsar should form channels running along their crests. The solid aqueducts we have experience of elsewhere are none of them very high, but are always breached and broken through after a time, when the river escapes and forms itself another channel, leaving the old bed meandering like a gigantic snake in the valley bottom. We cannot conceive such solid aqueducts remaining intact until they have been raised to a height of 300 or 400 feet.

Another difficulty presents itself when we compare *the contents* of the åsar with those of such river-channels as we can examine. Rivers which elevate their beds by gradual deposits are necessarily sluggish and slow-flowing rivers. When rapid, rivers become scouring agencies and not depositing ones. How is it possible to conceive of a sluggish river depositing these enormous masses of cannon-shot gravel — not of laying down a few yards of such gravel when there is an occasional rush in the stream, but a rampart a hundred miles in length and fifty yards high? The position is incredible. The Nile, the Rhine, the Indus, the Amazon, all these deposit beds, but they are beds of finely sifted mud. Again, in depositing stones, rapid rivers sift them according to their specific gravity, and do not mingle them higgledy-piggledy as they are mingled here. If it was a river that deposited these mountains of boulders, it must have been a very violent torrential river, and its force quite portentous along its whole course. If so, how is

it that it did not scour and move away all the sand and brickearth, and carry them down to its lower reaches, instead of laying them down along their whole course? All torrential rivers known to me have clean-washed, stony, and gravelly beds, with deltas or reaches lower down, formed of the lighter materials of denudation.

But in bespeaking torrential rivers of this kind in Sweden we are postulating a virtual impossibility. The level of Sweden is too low and too flat to afford such rivers. To get rapid rivers we must have steep slopes in their beds. Of course we have a rapid flow enough at places like Trollhättan, on the Gota river, and in other gorges where we have rapids like we have in the gorges of the Rhine; but there is no deposit like an åsar deposit in these gorges now. We cannot conceive any deposit of any kind long remaining in such places, nor does it seem possible that these gorges existed when the åsar were made. Elsewhere than at these gorges the rivers of Sweden are quiet and slow-moving, and deposit, not great masses of huge boulders, but sand and silt and mud. They must have been slower and less efficient as dynamical instruments when the level of the country was much lower, as apparently was the case in Sweden in so-called Glacial times. Again, the rivers of Sweden naturally flow from west to east, or N.W. to S.E., in channels in which they drain the upper plateau by running downhill to the sea, while, as we have said, the åsar run from north to south, right across the present river-channels and right across the lines of drainage of the country.

Again, rivers make deltas. When they have run their course, and get on to fairly level ground, they deposit fan-like stretches of mud and clay. There are no similar phenomena in the case of the åsar, which do not terminate as deltas at all, the flat spreads of gravel sometimes occurring in connection with them being torrential, and not like river deltas.

Rivers naturally have wider and wider channels as we move away from their sources to their mouths, and as their supply of water increases from their several feeders, and consequently as their loads of débris increase. This means that their beds become wider and deeper as we proceed downwards along their course. They are thus quite different to the more or less uniform ramparts called åsar, which chiefly differ in bulk in the fact that they are bigger at their initial stage than later on.

It seems absolutely impossible to correlate the åsar of Dalecarlia and those of Finland, some of which actually cross one another, and others are united by cross pieces, with any river-beds, whether subaerial or subglacial. Again, rivers of any size generally contain fresh-water shells or other débris. The åsar, on the contrary, when they contain shells at all, contain *marine* shells only. Rivers do not deposit marine shells.

Lastly, we must not forget that although we are considering the åsar as substantive phenomena apart altogether from other deposits, it is only for convenience of treatment. We cannot, in fact, separate the åsar and their contents from the sporadic and other deposits of

the same kind occurring elsewhere. The åsar are only heaped-up ramparts of materials which occur in the areas lying between them in a less prominent fashion, in some cases as scattered boulders, in others as continuous beds of sand and gravel and brickearth. Especially is this so with the brickearth or loam which often forms their upper layers. This is really part of the continuous mantle of the country. Such different deposits occur virtually at all levels. How is river action to account for these complementary phenomena? Rivers cannot spread over a whole country so vast as Sweden. They would cease to be rivers, and would become quite transcendental, like Baron Munchausen's dreams, and if they did so they would interfere with each other's beds, and the ramparts would have been levelled down. It is clear that in finding an efficient cause for the åsar we must find one which will also explain the deposit of the drift occurring outside them. Apart from and altogether beyond these difficulties is the supreme meteorological objection as to whence the rainfall was to come to fill these stupendous rivers, running parallel to one another, quite near together, and forming such a web of rivers as was never seen elsewhere. Where is the gathering-ground and where are the watersheds which could produce such a congeries of rivers? This is an important matter to those among us who believe in inductive methods in science. It is apparently of no consequence to those geological alchemists who are continually engaged in extracting palm-oil out of paving-stones. We cannot understand any meteorological or physical change which could supply the necessary rainfall for such rivers.

On every possible ground, therefore, known to me it seems quite impossible to connect the åsar with river action. This is not my view only; it was the view of my master, Murchison, also. He says: "However it may be argued that in mountainous tracts torrential rivers and their feeders may have descended as they do now, and may thus have produced rounded materials in valleys, the argument is, at all events, perfectly inapplicable to the formation of the Swedish åsar. These linear ridges have not only been accumulated in long *trainées* and lengthened mounds on terraces high above the valleys, but offer appearances entirely unlike those produced by rivers."

This view is, in fact, also endorsed by Professor James Geikie *in regard to subaerial rivers*. He says: "Banks of gravel and sand no doubt accumulate in the beds of rivers, but if the rivers were to disappear such banks would not form prominent ridges rising abruptly above the general level of the surrounding land. They would, moreover, coincide throughout any course with the lowest level of the valley, but our åsar, although they trend with the general inclination of the land, do not slavishly follow the line of lowest level, showing an independence of the minor features of the ground, sometimes winding along one side of a valley and sometimes along the other." ("Great Ice Age," p. 169.)

While Professor Geikie rejects Törnebohm's theory of the åsar having been the result of the action of subaerial rivers, he is willing

to accept the notion of D. Hummel and P. W. Strand that they may have resulted from the action of subglacial streams, and apparently also favours that of Dr. Holst, who assigns them to streams flowing over the surface of the ice. Now in regard to these theories, it seems to be forgotten by every glacial geologist that a subglacial river or a river flowing on the surface of a glacier differs from other rivers merely in that it flows under a long tunnel or archway of ice, or over a bed of ice instead of a bed of sand or gravel. In every other respect it is a river, and every difficulty which has been already pointed out in regard to the explanation of the åsar by river action of any kind is as potent and conclusive against these postulated glacial or glacier rivers as it is against ordinary rivers. In addition they present special difficulties of their own. Let us first look at the theory of Holst. It is quite true that when the sun beats upon the back of a glacier small streams are sometimes seen on its melting surface, which run for a few yards and then disappear down a crack or a crevasse. Nowhere, not even on the vast ice-plains of Greenland, do these small streams now grow into rivers; and in order to do so we must suppose that the ice was marked by no cracks or crevasses, and in the particular case of Scandinavia that in a singularly broken and uneven country an ice-mantle could exist without any crevasses or cracks draining its surface. But suppose it could, whence could it derive the materials for making gravel, or the great boulders, when the whole country was, *ex hypothesi*, blanketed with ice, and no exposed rocks were visible? And having got hold of rocky débris, how were these supraglacial streams to roll the millions of great boulders of granite, gneiss, and basalt, which form so large a part of the åsar, into their rounded and water-worn shapes, and accumulate them in dykes and embankments a hundred miles long and a hundred yards high? and how is it that the rest of the glacier's back or some part of it was not uniformly strewn with angular and unrolled, or with rolled débris, which should have remained when the glacier melted alongside of the åsar? Assuredly the whole idea is incredible, and it is incredible how sober, thoughtful men in our century should have tried to impose it upon science.

(*To be continued in our next Number.*)

## III.—Notes on the Affinities of the Genera of the Cheiruridæ.

By F. R. Cowper Reed, M.A., F.G.S.

In a former number[1] of this Magazine the evolution of the sub-genera of the single genus *Cheirurus* has been discussed, and it is now proposed to examine the mutual relations of the other genera of the Cheiruridæ. Some diversity of opinion has existed as to the genera which may be grouped together to form this family. Barrande[2] put only the following five genera into it: *Cheirurus*, *Sphærexochus*, *Placoparia*, *Staurocephalus*, and *Deiphon*.

[1] Reed, Geol. Mag., Dec. IV, Vol. III (1896), pp. 117 and 161.
[2] Barrande, Syst. Sil. Boh., vol. i (1852), pp. 335 and 766.

The last-mentioned genus was only provisionally united with the others. Salter,[1] while omitting *Placoparia* from the above list, added *Amphion*, which Barrande placed in the family containing *Encrinurus*, etc. The genus or subgenus *Sphærocoryphe* was also really included by Salter, but under the name of *Staurocephalus*? *unicus*. The genera *Encrinurus*, *Cybele*, and *Zethus* were also given by Salter as belonging to the Cheiruridæ, but with a query against each of them. Zittel[2] places the following genera in this family: *Cheirurus* (with its subgenera, *Cheirurus*, s.s., *Cyrtometopus*, *Sphærocoryphe*, *Crotalocephalus*, *Eccoptocheile*, *Pseudosphærexochus*, and *Nieszkowskia*), *Areia*, *Deiphon*, *Onychopyge*, *Placoparia*, *Sphærexochus*, ? *Crotalurus*, *Staurocephalus*, *Amphion*, *Diaphanometopus*. Of these, *Crotalurus* must certainly be at once removed to another family and group, because of the course of its facial suture.[3] More recently, Beecher,[4] in a valuable and suggestive paper on the classification of trilobites, has enumerated the genera and subgenera in this family thus: *Cheirurus*, *Actinopeltis*, *Amphion*, *Anacheirurus*, *Ceraurus*, *Crotalocephalus*, *Cyrtometopus*, *Deiphon*, *Diaphanometopus*, *Eccoptocheile*, *Hemisphærocoryphe*, *Nieszkowskia*, *Onychopyge*, *Pseudosphærexochus*, *Sphærexochus*, *Sphærocoryphe*, *Staurocephalus*, *Youngia*. Eliminating the subgenera we get the following: *Cheirurus*, *Amphion*, *Deiphon*, *Diaphanometopus*, *Onychopyge*, *Sphærexochus*, *Sphærocoryphe*, *Staurocephalus*, and *Youngia*. *Sphærocoryphe* must, in my opinion, be accorded generic rank. The genera *Placoparia* and *Areia* are placed by Beecher in the Encrinuridæ on account of their larval features, which suggest their union with this more primitive and less specialized family. In fact, he would apparently regard these two genera as morphologically the lowest in the phylogenetic list of the members of his order Proparia, which comprises the four families Encrinuridæ, Calymenidæ, Cheiruridæ, and Phacopidæ.

Omitting the imperfectly known and extra-European genus *Onychopyge*, we may concentrate our attention on the other genera which have been accorded a place in the Cheiruridæ, and all of which are found in Europe. The four genera *Placoparia*, *Areia*, *Amphion*, and *Diaphanometopus* are those whose true position is most a matter of doubt. Schmidt,[5] for instance, hesitates somewhat in retaining the two last genera in the Cheiruridæ; and the different views of Salter, Barrande, Zittel, and Beecher with regard to *Amphion* and the others have been mentioned above. The question can only be decided by the characters which one considers as essential to the family. But it is a matter of minor importance how we group together the genera in a system of classification, so long as we understand their phylogenetic relations. In *Areia*, in the first

[1] Salter, Mon. Brit. Trilob. Palæont. Soc. (1864), p. 2.

[2] Handbuch der Paläontologie (1885), vol. ii, p. 616.

[3] In Zittel's "Grundzüge der Paläontologie" (1895), this genus is omitted from the Cheiruridæ.

[4] Amer. Journ. Sci., vol. iii (1897), p. 89.

[5] Mem. Acad. Imp. Sci. St. Petersb., vol. xxx, No. 1 (1881): Rev. ostbalt. Tril., Abth. i, pp. 190, 195.

place, the absence of facial sutures separates it from all the other genera in the Cheiruridæ, while this feature, combined with the absence of eyes, the pentamerous lobation of the head, the expanded termination of the glabella, and the presence of the pleural row of puncta on the neck segment (in *A. Bohemica*), and the close resemblance of this segment to the thoracic segments are larval features which indicate a very low stage of development and a comparatively small amount of differentiation. The Bohemian species of the subgenus *Eccoptocheile* resemble it in the row of puncta on the inner portion of the pleura, the number of the thoracic pleuræ, and the notch on each side of the glabella in the front border of the cephalon; and Barrande himself remarks that *Areia* approaches most closely the species *Ch. claviger, globosus*, etc., belonging to the s.g. *Eccoptocheile*. The hypostome is analogous to that of *Cheirurus*, and the ornamentation of the cheeks is similar. As I have remarked in my previous article on *Cheirurus*, the pygidium appears frequently to follow an independent line and different rate of development to the other parts of the body, and is, therefore, of doubtful value in tracing affinities. In *Areia* it shows only two segments on the axis and two pairs of pleuræ. It is only in *Nieszkowskia* and in one species of *Sphærexochus* (*Sph. latens*, Barr.) amongst the Cheiruridæ that we find the pleuræ of the pygidium numbering two. But this does not show affinity, for in *Nieszkowskia* there has been reduction, absorption, and crowding out of the last pair by the hypertrophy of the first; in *Sph. latens* there has been fusion of the last pair of pleuræ into a terminal piece; but in *Areia* the second and third pairs of pleuræ alone seem to have been developed.

The fact that the Cheiruridæ show on the whole a high degree of differentiation and specialization, and have in the main lost the features of immaturity, while on the other hand the Encrinuridæ show a much lower stage of development and retain more larval or primitive characters, leads Beecher to place the genus *Areia* in the latter family. But from the several points of resemblance of this genus to the early species or subgenera of *Cheirurus*, and from its retention to maturity of certain larval and early phylogenetic features exhibited in some genera or subgenera which undoubtedly belong to the Cheiruridæ, I am inclined to place it in this family, and to regard it as a primitive form in which the ordinary ontogenetic development has been irregularly arrested.

Turning now to *Placoparia*, we find also in it many primitive features. The dorsal furrows are usually described as bifurcating in front, one branch running forwards and the other turning outwards at right angles. This latter branch may possibly represent one of the furrows we find on the cheeks in *Areia*. At any rate, the narrow marginal cheeks, combined with the absence of eyes, are primitive characters found only in the larvæ of higher trilobites. The facial suture ends also at the genal angle, recalling the less highly differentiated *Opisthoparia* of Beecher, in which the facial suture cuts the posterior border of the head-shield. The pentamerous

lobation of the glabella is also distinct, and this character, together with the presence of the ridge on the pleura, is considered by Barrande to unite the genus with the genera *Cheirurus* and *Sphærexochus*. But the former character is a larval one, retained to maturity in many utterly distinct genera, and the pleural ridge is shared by *Encrinurus*. The hypostome is entirely different to that of *Cheirurus*. The number of the thoracic segments (11–12), though the same as that of some species of *Cheirurus*, is in itself an almost valueless character. The pygidium alone is a treacherous guide, but its general resemblance to some of the *Eccoptocheile* group is worthy of notice. In *Pl. Zippei* there are five rings on the axis, but in *Pl. Tournemini* only four. Instability in the number of segments in the pygidium and thorax is generally characteristic of the less highly specialized forms.

It does not seem possible to establish any definite line of ancestry or affinity by which we may link *Placoparia* with other known genera; we must therefore regard it as an aberrant member of the family, diverging at an early period from the main stock, and not directly giving rise to any of the later forms.

The genus *Amphion* possesses a somewhat specialized head-shield, for the first side-furrows on the glabella are shifted forwards to the anterior border, and a small supplementary central furrow appears in some species between them. The presence of an epistome and pits on the cheeks are, as Schmidt says, points of resemblance to *Cheirurus*, but these characters are of different values. Eyes, though small, exist on the free cheeks, and the course of the facial suture is similar to that in *Sphærexochus*. The large and variable number of segments (14–18) in the thorax appears to indicate that the genus was still suffering evolution and was in a plastic condition, and the large number of body-rings alone is usually considered as a primitive feature. Thus Beecher has said that the indefinite multiplication of segments is to be regarded as a primitive character, expressive of an annelidan style of growth. On the other hand, the ridged pleuræ and the absence of any furrow on their surface recall *Sphærexochus*, and are apparently non-primitive features. The pygidium, with six rings on its axis and five pairs of pleuræ with free ends (of which the two last may be fused into one plate, as in *Amphion pseudo-articulatus*), reminds us of the multisegmented pygidia of some Encrinuridæ, and does not show any points specially associated with the Cheiruridæ. Barrande placed this genus with the Encrinuridæ chiefly on account of its numerous body-segments, but from the above review of its main characters the evidence for its location amongst the Cheiruridæ appears to be stronger. It is clear, however, that it branched off from the main stem at an early period, and pursued a somewhat independent line of development and specialization. Its stratigraphical antiquity also leads us to place its divergence at an early date.

The Russian genus *Diaphanometopus*[1] seems allied to it; the

[1] Mem. Acad. Imp. Sci. St. Petersb., vol. xxx, No. 1 (1881): Rev. ostbalt. Tril., Abth. i, p. 195.

head-shield shows many points of resemblance; the pygidium is similar, and the short furrow on the anterior edge of the inner portion of each pleura of the thorax may possibly correspond to that in *Amphion*, which Salter says exists beneath the crust. There are only twelve body-segments in the single species which has been recorded. From want of material for examination I cannot say more about its affinities.

The remaining genera can have their relations indicated with more certainty. The genus *Sphærexochus*, as described in my previous article, is led up to by *Pseudosphærexochus*. In the latter the posterior branch of the facial suture cuts the outer margin of the head-shield only a short way in front of the genal angle, and the basal lobe of the glabella is incompletely circumscribed; in *Sphærexochus* the suture cuts the margin at the genal angle itself, and the basal lobe is completely circumscribed by a strong furrow. In *Pseudosphærexochus* the body-segments are twelve in number and the rounded pleuræ bear a nearly obsolete line of puncta; in *Sphærexochus* the body-segments are reduced to ten,[1] and the last traces of the puncta have completely disappeared; the pleuræ also are shortened by the reduction of the length of their outer portion. In *Pseudosphærexochus* the pygidium shows four pairs of pleuræ with long pointed free terminations; in *Sphærexochus* the number is reduced to three—or even to two in one species,[2] *Sph. latens*—the last pair or pairs having disappeared, perhaps by fusion followed by absorption, as suggested by *Sph. latens* and by *Sph. bohemicus*,[3] where the second pair forks.

It is especially interesting to find in this highly specialized trilobite—the culmination of one line of differentiation of the Cheirurid stock—that there is a tendency to revert to the more primitive condition of the Opisthoparia by the migration backwards towards the hind border of the head-shield of the marginal point of section of the posterior branch of the facial suture. The free cheeks are connected by a narrow linear band representing the epistome which is apparently fused with them, for the facial sutures unite in front of the glabella and no twin sutures connecting them with the hypostomal suture have so far been discovered. It would seem as if the obliteration of these connecting sutures was another fact in evidence of the high specialization of the genus. I do not attach excessive importance to the fact that the cheeks are only ornamented with fine tuberculations, and lack the characteristic pits of the typical Cheiruridæ, for we know how completely well-defined and important surface markings, such as pleural and glabellar furrows, may become faint or disappear, how the pleural puncta of some forms may gradually be obliterated, and how tubercles may vary in size and be present or absent in closely allied species. Barrande has figured and described the hypostome of *Sphærexochus*,

[1] Salter (Mon. Brit. Trilob., p. 78) says there are eleven segments, but only figures ten.

[2] Barrande, Syst. Sil. Boh., vol. i, Suppl., p. 113, pl. ix, fig. 3.

[3] Ibid., p. 112, pl. vii, figs. 5, 6.

and it can easily be derived from that of a typical *Cheirurus* by a broadening and flattening of its several parts.

Turning now to another branch of the family, we find in *Sphærocoryphe* the forerunner and ally of the peculiar genus *Deiphon*. The relations of *Sphærocoryphe* to *Cyrtometopus* have previously[1] been discussed. Some of its leading characteristics we find repeated but more accentuated in *Deiphon*. Thus, in *Sphærocoryphe* the enormously inflated anterior portion of the glabella and the faintness of the first and second side-furrows foreshadow the condition found existing in *Deiphon*, in which the two pairs of furrows have disappeared and the glabella has a regular globular form. In several species of *Sphærocoryphe* (as, for instance, *Sph. Hubneri* and *Sph. unicus*) the basal lobes of the glabella are distinct, but in *Sph. cranium* they are very faint, and thus prepare us for their complete absence in *Deiphon*. Again, in *Sphærocoryphe* the free cheeks are merely small triangular plates, bearing the eyes, wedged in on the anterior border of the fixed cheeks; in *Deiphon* the reduction in size of the free cheeks has proceeded so far that they are only represented by the eyes and a small part of the doublure of the head-shield. The fixed cheeks in *Sphærocoryphe* bear one or more tooth-like processes on their front edge; in *Deiphon* the fixed cheeks are so narrowed and modified as to form long spines, but they still bear the tooth-like process on their front edge, as in *Sph. granulatus*. In the thorax we find on the pleura in *Sphærocoryphe* a longitudinal furrow, and in the Bohemian individuals of *Deiphon Forbesi* there is also a similar, though fainter, furrow present.[2] The reduction in the number of the body-segments to nine is certainly in this genus, as in *Sphærexochus*, a sign of high specialization when we consider it in conjunction with its other characters, but the loose build of the body and absence of fulcrum seem to be reversions to the more archaic types, and to represent the gerontic and degenerate stage in the phylogeny of the group. The migration of the eyes forwards to the anterior edge of the head-shield is also distinctly a retrogression to the larval condition, for it has been proved[3] that the eyes first appear on the anterior margin of the dorsal shield in the protaspis, and move backwards in subsequent stages of growth.

The pygidium in *Deiphon* has only one pair of pleuræ—the first pair; but this pair is enormously developed. The pleuræ of the other segments are aborted; but four segments are traceable on the axis. This great development of the first pair of pleuræ is likewise foreshadowed by *Sphærocoryphe*, for in *Sph. unicus* the first pair is much enlarged, while the posterior ones are reduced. There seems

[1] Geol. Mag., Dec. IV, Vol. III (1896), p. 118.

[2] Salter says (Mon. Brit. Trilob., p. 88) that the pleuræ are ungrooved, but in his figures of the species (ibid., pl. vii, figs. 1-12) furrows are shown. Barrande both describes and figures the furrows (Syst. Sil. Boh., vol. i, Suppl., p. 115, pl. ii, figs. 19, 20). Perhaps, as Barrande suggests, for this and other reasons the Bohemian and English species are not identical.

[3] Beecher, Amer. Geol., vol. xvi (1895), p. 177, and references.

to be a frequent tendency in the Cheiruridæ for the posterior pairs of pleuræ to be reduced, and we find it remarkably exhibited in *Nieszkowskia*. In regard to *Deiphon*, though the morphological value of its various characteristics may be a matter of dispute, yet its high degree of specialization must be generally acknowledged, and we would place it at the end of this branch of the family. Beecher[1] has also briefly stated that he is of this opinion, and he associates with it the Australian genus *Onychopyge*.

Turning now to the aberrant and imperfectly known form *Youngia*, distinguished by Lindström[2] as a separate genus, we find there are only three species established, and of these only the head-shields have been discovered. One of them (*Y. trispinosus*) is found in the Penkill mudstones of the Girvan district. The characters of its head-shield link it on the one hand with *Pseudosphærexochus*, and on the other with *Sphærexochus*. The spiniform fixed cheeks recall *Deiphon*, but their development is not so extreme; the neck spine is a marked feature, but does not seem to be of phylogenetic importance. In some species of *Acidaspis* and *Lichas* a spine is developed in the same place, while other closely allied species are destitute of this ornament. Bernard[3] has compared this organ, which is often a mere tubercle, with the dorsal organ of *Apus*, and suggests that it was poisonous.

Lastly, the genus *Staurocephalus* demands our attention. This genus, though frequently stated to be allied to *Sphærocoryphe* on the strength of its abnormal glabella, possesses in reality many important distinctive features. The hypertrophy of a portion of the glabella has in my opinion more of a physiological than of a morphological value. In the style and extent of the inflation of the glabella of these two genera a considerable difference is found to exist on careful examination. In *Staurocephalus* it is only the frontal lobe which is inflated and projects so conspicuously over the front border of the head-shield; the first side-furrows are very strong, and unite across the glabella in a continuous groove, thus sharply marking off the globular frontal lobe. The hinder portion of the glabella is parallel-sided, and has two pairs of furrows marking off the lobes distinctly, but the basal lobe is not circumscribed. In *Sphærocoryphe* excessive development is shown by the glabella as a whole, with the exception of the basal lobes, which are in the form of nodules. The facial suture in *Staurocephalus* cuts the outer border of the head-shield posteriorly, so that the free cheeks are of a fair size, but in *Sphærocoryphe* the free cheeks are relatively very much smaller and of a different shape, owing to the forward course of the posterior branch of its facial suture. In fact, the two branches of the facial suture in *Sphærocoryphe* meet at a very acute angle, while in *Staurocephalus* they meet at almost a right angle. This feature, from what we now know of the ontogenetic and phylogenetic history

[1] Amer. Journ. Sci., vol. iii (1897), p. 201.
[2] Öfver. Kongl. Vet. Akad. Forhandl., 1885, No. 6, pp. 49–51, T. xiii, figs. 11, 12.
[3] Bernard, Q.J.G.S., vol. L (1894), pp. 422-4.

of the free cheeks, must be taken as of considerable importance. The marginal spines on the head-shield of *Staurocephalus Murchisoni* may perhaps be regarded as merely the multiplication of the one or two pairs present in *Sphærocoryphe*, but a marginal fringe of spines is of but little morphological value, for we find it in the most widely separated forms, such as *Areia* and *Acidaspis*, as well as in closely allied species. The cheeks of *Staurocephalus* are not pitted like most of the Cheiruridæ, but are tuberculated, and in this respect, as well as in the stalked eyes, resemble some of the Encrinuridæ. But surface ornaments may have too much importance attached to them, and I doubt if they are often of more than specific value. The thoracic segments show a considerable resemblance to those of some genera of the Encrinuridæ in their shape, their rounded and ridged surface, and the fulcrum, beyond which they are sharply bent down. These features are suggestive, and seem to be of considerable value in this case in indicating the relationship of this strange genus. A furrow exists on the anterior edge of the pleuræ of *St. Murchisoni*, as Salter described, and a similar one occurs in the same position in *Encrinurus*. This furrow may perhaps not be homologous with the ordinary diagonal or longitudinal pleural furrow of other genera, but it is a common feature in *Encrinurus* and *Staurocephalus* which is worthy of notice. In *Sphærocoryphe* the thoracic pleuræ are not ridged, and the furrow which is present on them runs along the central line. However, we are not precluded from supposing the possibility of its obliteration when we recall the case of *Sphærexochus* and *Pseudosphærexochus*. In the pygidium of *Staurocephalus* we see again the impracticability of deciding affinities by this member. The pygidium of one species (*St. globiceps*) shows a close resemblance to that of *Sphærocoryphe unicus*, and in each form the first pair of pleuræ is much enlarged at the expense of the others. But the less specialized pygidium of *St. Murchisoni* is completely different, and while retaining the Cheirurid character of a small number of segments, yet in its general shape, and the form and course of the pleuræ, it strikingly reminds us of some species of *Amphion*, and, more remotely, of some species of *Cybele* and *Encrinurus*. Barrande has remarked that its pygidium shows a particular analogy to that of *Ch. tumescens*. From the foregoing consideration of some points in the anatomy of *Staurocephalus* I am led to conclude that its affinities are rather with the Encrinuridæ than with the Cheiruridæ, and that its resemblance to *Sphærocoryphe* is more superficial than real, and is probably an instance of isomorphism. The pygidium in any genus is too variable a feature to be of much use in determining true relationships, but in the simplest and least modified form of this member, as represented in *St. Murchisoni*, it is deserving of notice that there are indications of an alliance with the Encrinuridæ. It may here be remarked that in the latter family the large number of segments on the axis of the pygidium probably does not represent so large a number of coalesced pygidial segments, but is due to the secondary subdivision of the original segments. Finally, we must come to the

conclusion that *Staurocephalus* diverged from some early Encrinurid, and while retaining several of its ancestral characteristics underwent a development under somewhat similar conditions as *Sphærocoryphe*, leading to similar adaptive changes in certain points of its structure.

## IV.—A Revindication of the Llanberis Unconformity.

By the Rev. J. F. Blake, M.A., F.G.S.[1]

(*Concluded from the April Number*, p. 178.)

### *The District South-West of Llyn Padarn.*

THIS is undoubtedly the most difficult district to deal with, and one in which I have had to change my views on certain details. Still, there is one part of it where the evidence is very clear, namely, the ground between the railway and the road at the Tan-y-pant inlet. This was apparently not examined by Professor Bonney when he visited the locality, and its teaching is evidently not appreciated by his coadjutor. They say that the section at what they call "the supposed junction" is undoubtedly very difficult. One thing, however, is clear, since we all agree about it. The junction of a rock of felsitic character with one like a purple slate is *vertical*. There are here only two alternatives: either the felsite is intrusive into the purple slate, as the junction at first sight certainly suggests, or the purple rock was deposited on the felsitic one. In spite of appearances, we agree to reject the former alternative; but my critics seem to think they are correcting me when they agree with me in accepting the latter. The only possible difference between us is, that what I called felsite, including therein, as I remarked in a note, "felsitic ash," they call "felsitic grit." This difference, which, as far as I am concerned, is merely one of words, is quite irrelevant to the argument that the junction-line between two deposits must have been at first approximately horizontal, and have been subsequently turned on end. But the neighbouring conglomerate lies in a hollow on the present upper surface of the felsite, and must therefore have been deposited after the rocks had been turned on end, and therefore be of later date than the slate. (See Fig. 1.)

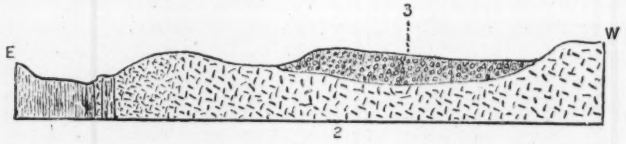



Fig. 1.—Relations of slate (1), felsite and associates (2), and conglomerate (3) at the inlet by Tan-y-pant, Llanberis.

It is true that my critics draw a fault somewhere about here, but it is not clear where they think it runs. It cannot be in the slate, as that would throw no light on the question; it cannot be at

[1] The substance of a paper read to the Geological Society on December 1st, 1897, with additional remarks.

the junction, which they describe as a surface of original deposit: it must be in the felsitic part, probably at some supposed junction of felsitic grit with true felsite. Such a fault would have to rotate the conglomerate and bring it out from between the felsitic grit and felsite. Now, the rocks in this area are quite bare, and one can see for certain that there is no fault; even the mass that discloses the junction with the slate is continuous till it becomes a true felsite. The only possible fault in the neighbourhood is on the other side of the mass of purple slate between it and the next exposure of conglomerate, that is, along the face of the cliff above and below the road.[1]

The similar section described by Sir A. Geikie (E, p. 96) is doubtless on the continuation of the line of junction seen below. The purple slate[2] is there also said to be nearly vertically interbanded with felsitic material which passes towards the west into a true felsite. Sir A. Geikie does not say exactly where this section is, but describes it as separated from "the porphyry of the ridge" by a "zone of conglomerate and grit," so it is most likely where Miss Raisin has inserted felsite on her map. In my map, however, it is included with the "Post-Llanberis," because on the upper surface there are scattered here and there some pebbles of quartz, so that it is covered, as it were, by a skin of conglomerate. Here again, then, if the spot is rightly identified, the plane of junction with the slates is nearly vertical, while that with the conglomerate is horizontal.[3]

This inlet section is really an important one, as it is the only place I know of, except in tunnels, where the felsite and any other rock than the conglomerate can be seen in unbroken sequence. What we see in such a case I regard as *one* irresistible argument for the unconformity I postulate, and, so long as it holds, the question whether the purple slate here is the workable slate or not is of secondary importance, for if the conglomerate is unconformable it may just as well extend over the workable slate as not. The rock is like the workable slate, and like no rock out of that group; as a fine-grained rock it cannot be a mere local deposit; and it is in continuation with the worked slate which runs over the hill to the south-west. My critics also recognize in it one of the characteristics of parts of the workable slate series—the "interbanding of fine grit and purple slate," which is quite a distinct thing from the "alternation of hard grey slate, transversely cleaved, and coarse grit," characteristic of the Post-Llanberis group.

[1] Professor Bonney and Miss Raisin also claim a fault on the other side of the conglomerate, between it and the main mass of the felsite, but the junction may be seen in a block near the water's edge; one is welded to the other.

[2] So gradual is the passage from one rock to the other that Sir A. Geikie considers the parts of the slate nearest to the felsite to be only a cleaved portion of the latter.

[3] A similar argument is applicable to the tramway section on the opposite side of the lake. The felsite there shown next the first conglomerate is followed beyond the conglomerate (and a dyke) by nearly vertical slaty beds, but the summit of the felsite crag shows a covering of conglomerate, indicated by the presence of quartz pebbles. Here, however, the vertical succession is broken.

Next, they cannot believe that the conglomerate passes over to the other side of the purple slate, because the rock there found seems to them to be less squeezed, more purple, with a few additional varieties of pebbles, and thinner. These differences (excluding the first, which one cannot deal with seriously) are slight at the best, and just what we should expect in a shore deposit, the change in colour being related to its position over purple slate rather than over felsite.

Next, in relation to the synclinal in the railway cutting and my belief in its unconformity, Miss Raisin says that the purple slate "turns up again" on the south-east side of it, and thus behaves as the overlying strata do. But the phrase quoted is ambiguous; it may be intended as a colloquial expression for "occurs," or it may mean that the dip is changed. It is quite to be expected that slate would *occur* where she has marked it, and, indeed, I have it so in my note-book section, here copied (Fig. 2), though it is omitted in my general section of this cutting, but I could find no proof of dip in it, while in the nearest visible purple slate I noted a high dip to the E.S.E. as on the other side of the synclinal. But an unconformity or its

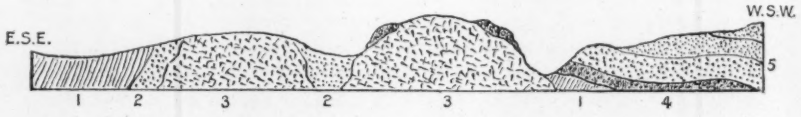



FIG. 2.—South-east end of the synclinal in the Llanberis railway cutting. (1) Purple slate, (2) green (St. Ann's?) grit, (3) greenstone, (4) conglomerate, (5) grit.

absence cannot be proved in this sort of section; it is too obscure. Its only use is to show how far an unconformity, otherwise proved, extends over the underlying rocks. Still, what we see here is more favourable to an unconformity than the reverse. I pointed out that the conglomerate "mounts up over the back of the greenstone boss" and passes to the other side, as now shown in Fig. 2. This looks like a transgression over the outcrop of slate.[1]

I also described the conglomerate as leaving the felsite along the north-west boundary and disclosing between them a different succession. I briefly described the immediate successor of the felsite here as a hard purple slate, but Miss Raisin goes into details and records between the felsite and conglomerate a breccia, a pebbly grit, a banded grit and argillite (a greenstone), and a purple grit,—just what we might expect to follow the felsite in spots now further west, representing hollows on its surface at the time of their deposit.

[1] The following quotation from Professor Bonney's paper (C) will show that he at that time gave quite a different account of this section from mine and Miss Raisin's, and considered the conglomerates now distinguished as inseparable except as varieties. "The cutting . . . . passes into fine green grits or 'bastard slate,' beyond which we find a thick mass of interbedded conglomerate and similar grit, then another band of grit, followed by a band of small rolled fragments of felsite about as large as hemp-seed." It is plain that either what he here calls fine green grit or bastard slate is the same as that now called purple argillite, or he has missed the first conglomerate in the cutting, and takes the next band to be the "Cambrian" conglomerate, calling it "the finer variety," and saying that "with considerable variety of detail the general character of these is similar."

Their occurrence indicates a long interval between the outflow of the felsite and the deposit of the conglomerate, and this interval must include an unconformity, since the conglomerate comes back again and lies on the felsite in a very short distance. My critics try to account for these numerous intervening beds, each indicating some change of conditions on the same spot, by reminding us of the interchangeable deposits of grit and coarser material on the seashore. This I have already allowed for, having included with the conglomerate much that is only the red grit into which it passes in places.

In connection with the strata which overlie this conglomerate, I am charged by my critics with inaccuracy when I say, speaking of their lamination, that almost wherever seen these laminæ are horizontal. No doubt from its form this statement is liable to be taken to mean that the strata are almost always horizontal. That this is not the meaning might be inferred from the high dips shown in my section of the railway synclinal, which must necessarily be continued into the country behind. They are, in fact, seen, as stated by Miss Raisin, in many places. But the question is whether in places where the dips are high the laminæ can be seen. Possibly they can; I have, however, seldom found them, except where the strata are nearly horizontal, either in this district or higher up on the hill, or on Y Bigl, and even Miss Raisin only speaks here of high dipping "outcrops," but when elsewhere "lamination" is mentioned it is accompanied by the word "horizontal" (loc. cit. (K), p. 588).

These horizontal laminæ are for the most part only observable on the summits of elevations, such as the spots I named (10, 16, 18, on my map), the last one being both the highest and best; it is marked 666 on the 6 inch ordnance map. How such horizontal laminæ above are compatible with highly inclined outcrops below, is explained by the deposit of the strata on an undulating surface. Those lying in the hollows would be squeezed into synclinals under earth pressures, being nipped in between the older and harder rocks, while the higher ones might escape.[1] Thus the high dipping outcrops in the low ground are no arguments against the general horizontality of the strata, and therefore call for little notice. If Miss Raisin had given the direction of the dips she noticed we might find that their average was zero. I ought to remark, however, that my words "dip" and "horizontal" here have reference to the *apparent* dip in a direction parallel to the lake-shore. There may be also, and probably is, more or less of a dip towards the lake in the direction of the strike of the underlying slates.

We now pass to the strictures on my statement about the grounds of Glyn Padarn. They say it is not easy to recognize my "definite succession along an E.N.E. and W.S.W. line." Miss Raisin, however, has succeeded in doing it and in confirming my statement. But in quoting me my critics have omitted the words which I actually

[1] The manner of this was illustrated by a model at the reading of my paper.

italicized as containing the gist of the matter, that this line is the line of strike of the purple slates. Is it necessary to explain that if these surface beds are bands in the slate series, and we go along the line of strike, we must continue on the same bed, and not change again and again? The definite succession is fatal to the hypothesis. It indicates a strike at right angles to the dominant strike of the district, which is only possible in superficial deposits. Even a fault cannot do much towards altering the strike, unless it be on a geotectonic scale.

The dips Miss Raisin records in some part of these grounds are of no consequence from this point of view, though *combined* they give a general "gentle dip towards the E.N.E.," and separately they indicate a slight synclinal arrangement. But in the centre of the grounds, standing with one's back to the road and looking along the strike of the slates of the country as shown in the neighbouring quarry, we see the crag shown in Fig. 3. It illustrates at the same time the

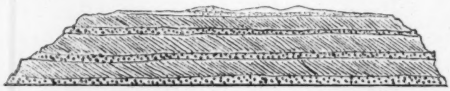


Fig. 3.—Low crag in the Glyn Padarn grounds, looking W.S.W.

perpendicularity of the strike of the beds (whatever be their dip) to that of the slates, and my description of the alternations of transversely cleaved grey slate and coarse grit characteristic of the Post-Llanberis group. As noted above, a fault between this and the quarry would make little difference to the argument, but of such a fault there is absolutely no evidence, and in the absence of one the bottom beds of worked slate in the quarry, if continued on their rise, would run into this crag, or very near it. The transverse fault seen in the quarry has nothing to do with this, but lies beyond.

In the large exposed surface of conglomerate "beyond the wall" a band of purple slate most certainly lies with a very low dip upon

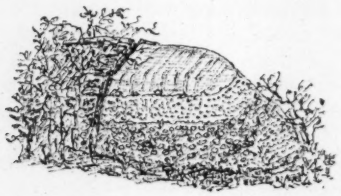


Fig. 4.—Mass of purple slate mud, overlying conglomerate outside the Glyn Padarn grounds, looking N.E.

it. This band is perhaps more probably a redeposited purple mud than a fragment. It is shown in Fig. 4. The conglomerate, *pace* Miss Raisin, does contain felsite pebbles, and in no way resembles the St. Ann's grit, or any grit interbanded with purple slate

elsewhere, for, as desired by my critics, good "distinctions between the various conglomerates and grits" have already been drawn, and in most cases, as here, can be easily recognized.

I regard all this as very strong evidence that there are unconformable deposits here, and cannot see how it can be affected either for good or ill by anything that may be seen in the railway cutting. Nevertheless it is satisfactory to show how the neighbouring exposures can be explained in relation to our conclusions. Thus on the north-west side of the grounds we find purple slate and ordinary St. Ann's grit coming on again at a higher surface-level. This I explain by the hypothesis of a fault which has let down the unconformable deposits to a lower level than they originally occupied. But to support this hypothesis we should be able to point out the fault and show that it lets down something—we cannot reasonably expect any particular kind of rock — on the S.E. side. Such a fault we can point to: see Fig. 5. This suggests at least, by the anticlinal

Fig. 5.—Fault seen in the Llanberis railway cutting, looking N.E. (1) Purple slate, (2) green slate, (3) slickensided fault, (4) conglomerate, (5) grit.

structure on the left, that it is a down-throw on the right, and, as it happens, on this side are rocks, conglomerate and grit, which, from what we have observed above, could easily be brought here by a slight fault.

In reference to this my critics say: "Mr. Blake speaks of it as a slight fault, but it is only so on his own hypothesis, and to argue from that statement is reasoning in a circle." But I have not argued from it at all. If I were to do so now it would be to say that the hypothesis, that this conglomerate has been let down from above by a slight fault and forms the continuation of similar rocks near at hand, is a less wild one than that which supposes it to be "faulted up from great depths," and so have no relation to the neighbouring conglomerates. This last hypothesis is not even suggested by anything that is seen on the ground.

Before leaving this district I should point out that the map of it given by my critics is inconsistent. They draw a fault by the Tan-y-pant inlet, and make the succession on the two sides of it different. On one side it is felsite, conglomerate, felsitic grit, grit with argillite; on the other, felsitic grit, purple slate, conglomerate, grit with argillite. These two successions cannot both be the true one.

### *The Tramway Section and Y Bigl.*

I will not here, as I did at the reading of my paper, explain the corrections I have been led by additional observations to make in my own map, as they lead me further from my critics' views, but will confine myself to their statements.

We will commence with the tramway section. Of this Professor Bonney gave a diagram in C. He and Miss Raisin now give a second, and they say, "we adhere to our original diagram." If anyone will take the trouble to put these two diagrams (A and B) side by side and compare them, he will notice that (1) in A a fault is placed between the felsite and the conglomerate, in B it is shifted to the other side of the conglomerate; (2) in A purple slate is represented as occurring in the synclinal, in B there is none; (3) in A the middle conglomerate is undivided, in B it is divided into two; (4) in A the beds between the second and third conglomerate are called green slaty grit, in B they are quite differently described; (5) in A these beds are represented as forming an anticlinal, making the second and third conglomerates parts of the same bed, in B these beds are all made to dip towards the second conglomerate, making the third a distinct and lower bed. When, then, the authors claim that they adhere to the original diagram, and add, "but would shift the anticlinal so as to fall nearly on the third conglomerate," they might as well have said, "but change it altogether."

I do not complain of a change of views, if frankly acknowledged, but in this case I fear it is a change for the worse, for it lands them in many difficulties. *The* Cambrian conglomerate has gone by the board, and they have become quite lavish of their successive conglomerates. They have already claimed one below that on the summit of Moel Tryfaen, and one above that next the felsite by the Llyn Padarn inlet, and here there must be a fourth unless they can account for the change of material lying between the supposed two on one side of the lake, and the two on the other. This material in one case is felsitic grit and purple slate, and in the other felsitic grit of a different kind and "rainspot breccia." Next they have to account for the absence of the second conglomerate on the north-west side of the banded-slate synclinal, and to this end have to introduce a new hypothetical fault for which there is no independent evidence, but rather the reverse. Then they have to deny the identity of the very similar conglomerates in the two crags almost facing each other on the slopes of Y Bigl, on the sole ground that one of them contains additional varieties of pebble. At the same time they identify one of these, on the west of Moel Goronwy, with a totally different felsitic breccia, containing none of the great pebbles, on the east side. They have to make two conglomerates along the west side of that hill, where they acknowledge not to have examined the ground, and where there is certainly only one; and they draw the second conglomerate in a straight band right over the high ground, thereby representing it as vertical, though in the tramway section they state that it has a moderate dip.

There is, however, direct evidence that these second and third conglomerates are parts of one and the same sheet, for one can be traced round on the slopes above the section, passing across about the level of the lower road, with a very narrow interval of grass, into the other. This, while it negatives the new section, does not prove an unconformity; for if, as Professor Bonney originally

represented, there were an anticlinal here, the junction would naturally take place over its crest; but of such an anticlinal no one, from Sir A. Ramsay onwards, has ever been able to find a trace, and it is now abandoned even by Professor Bonney and his coadjutor. From these considerations and others to follow I hold that an unconformity is the only alternative left, whatever may be the teaching of Professor Green's section.

This section, seen on the tramway, has been differently interpreted by my critics and myself. If my opponents' reading of it be correct, I should say with one of them that the unconformity was only locally absent; if mine be correct, it will afford only confirmatory evidence, valuable, no doubt, but not essential. My critics, however, try to give it an unmerited importance. They "are told," they say, "that here the hypothesis can be brought to a direct test." They were certainly never told so by me. No hypothesis can become a conclusion without being subject to many tests, and a single section, with the possibility of a local absence of unconformity, is not suitable for a "final appeal."

To me the evidence of this section is clear enough, but Sir A. Ramsay, Sir A. Geikie, and Miss Raisin are all against me, and I have only the support of Professor Green,[1] a good stratigraphist. With regard to this support, it is not fair of my critics to say that "the only line which might be supposed, and has been supposed by Professor Green, to mark it [the unconformity] is not shown by Mr. Blake," when I distinctly said that "his description is so wonderfully true to nature that I could only quote him verbatim," including, of course, his diagrams. My general diagram was intended only to illustrate my view of the general relations, just as was Professor Bonney's.

I do not think the line which those who see conformity here have noticed some way down in the breccias is really a line of bedding. I can see there no change of material, but of course I may be wrong. But if so, there still remains the cleavage. Now cleavage, as Dr. Hicks pointed out, had already been produced in the district before the conglomerate, containing cleaved pebbles, was formed. Nor do I see how the angular fragments in the underlying breccia could have been turned round, thereby shortening the horizontal dimension of the rock, after the conglomerate was there, and yet leave an undulating, closely-welded line between the two rocks, seeing that the conglomerate has not been shortened, and the line of junction is, at least in places, perpendicular to the cleavage. Hence, whether from bedding or cleavage, I think the breccia fragments were made vertical before the deposit of the conglomerate.

Again, my critics say that "the 'nearly horizontal line' does not exist." As to this, it must be remembered that for two rocks to be conformable they must be conformable everywhere, but to be unconformable they need only be unconformable anywhere. It is thus quite enough for the purpose that my critics themselves

[1] Professor Hughes and Dr. Hicks *accepted* the unconformity, but I do not know that they have personally observed it.

draw a festoon on the boundary, part of which must of course be actually horizontal. It was, in fact, from this more horizontal portion of the boundary that I was judging when I wrote. But if the amount of dip be in question, none of the drawings yet given of the section are accurate enough. The two rocks are broken up by a number of transverse joints, along which slips appear to have taken place, producing a step-like series of festoons (see Fig. 6). If these were restored, the slope would not appear so great.

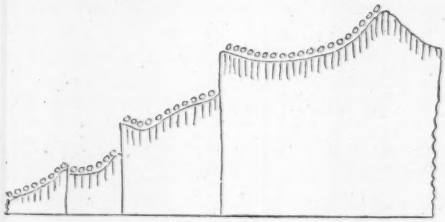


Fig. 6.—Junction of conglomerate and breccia in the tramway cutting, Llyn Padarn, looking N.E.

My critics further state that "the unconformity is quite disproved by the finer matrix graduating from one [the breccia] into the other [the conglomerate]." In a hand-specimen the line is sharp, and the two matrices are differently coloured. In the field the joint face of one is smooth, being along a cleavage plane; of the other, rough, where the cleavage almost ceases. But a similarity of matrix is to be expected when one rock is derived from the other. This is perhaps the last district where such a similarity should be used as an argument, for here we have the well-known example of even a crystalline felsite yielding to the succeeding conglomerate a matrix which can be scarcely distinguished from the original rock. When the original is clastic we may well expect the derived matrix to be absolutely indistinguishable. The fallacy, too, of this argument has been already demonstrated. On this ground the conglomerate overlying the "granitoidite" at Twt Hill was positively asserted to belong to the same series, but it was afterwards acknowledged to be of a totally different age and unconformable.

I do not know that this section is worth all this labouring to prove its teachings. I am only forced to it by the dogmatic tone adopted by my opponents. I should rather gather from the conflict of opinion here how very useless a single section may be to carry conviction as to the structure of a country; and in this case the further evidence enables us to dispense with it altogether.

There is, indeed, in this district a discriminating test as to the truth of my reading or of that of my critics, which may be applied again and again in many places and in many ways. Immediately to the north-west of Green's section lies a synclinal of pale banded slates and grits, towards which, according to my opponents, the conglomerate is dipping, and which they take to overlie it. That is,

the slates are *above* the conglomerate. According to my reading, the conglomerate is unconformable over them. That is, the slates are *below* the conglomerate. Here, then, is a clear issue. As to this, my opponents state as "further proof" of their view, that "the grits continue the succession above the conglomerate," and "rain-spot breccia [i.e. rock like the underlying breccia at Green's section] recurs above the supposed unconformity." Now, if the authors could point to any clear section in which a rock undoubtedly belonging to the banded-slate series, whether a grit, a breccia, or a slate, can be seen overlying Green's conglomerate in regular sequence, it would be fatal to my account of the rocks. This ought not to be a difficult matter, somewhere along the line of junction depicted on their map, but they do not point to any such section; they use the ambiguous phrase "continue the succession." If they cannot do so, but only *assume* that the "grit" and "rain-spot breccia," from their position to the north-west, i.e. in the direction of the supposed dip, of the conglomerate, must lie above it, the circularity of their "proof" leaves nothing to be desired.

On the other hand, I have searched in vain for such a section, but have only found patches of conglomerate overlying slates, as figured by Professor Green, not only immediately above his section but also to the north-west of it. The ground about here, however, is low and hummocky, and does not yield good sections. Further up the hill it is more hopeful. Above the Fachwen road the junction runs obliquely up the hill, making a large angle with its former direction. According to my opponents' explanation, it is the lower beds here that form the higher ground, which they account for by giving the synclinal axis a dip towards the lake.[1] Along the line of junction here there are some clear sections which are so fatal to my critics' views that I am sure Miss Raisin cannot have seen them, as she could never have suppressed them if she had, especially as I alluded to them in my former paper. They are shown in Fig. 7. They are found a little to the north-east of the spot where I marked + on my map. The first is near to the angle of a wall, where it curves round as shown on the 6 inch ordnance map. They cannot be missed, and they speak for themselves. The drawings represent nearly vertical faces, and the sides of the crags show that the conglomerate is not carried down behind the slate. The underlying slates are continuous as far as can be seen down to the tramway, and the conglomerate is of the same type as all three in that section. Other similar sections continue the line till we are removed only by a narrow valley from the continuation of the conglomerate adjoining the felsite, the only visible rock intervening being a boss of greenstone. I claim that the test proves my reading to be right.[2]

[1] They do not explain how they get the synclinal down again into the further valley consistently with their mapping of the beds; nor do they account for the enormous expansion of the strata between the conglomerate and slate compared with the tramway. Y Bigl summit is also represented as on slates, and not on laminated grits, but this may be an unintentional error.

[2] It is also in accordance with Sir A. Ramsay's section.

In my paper I said that these conglomerates crossed the hill horizontally, and at this statement my assailants make themselves merry. No doubt it would have been more correct to say that they form a slight synclinal, as indicated on Sir A. Ramsay's figure in his

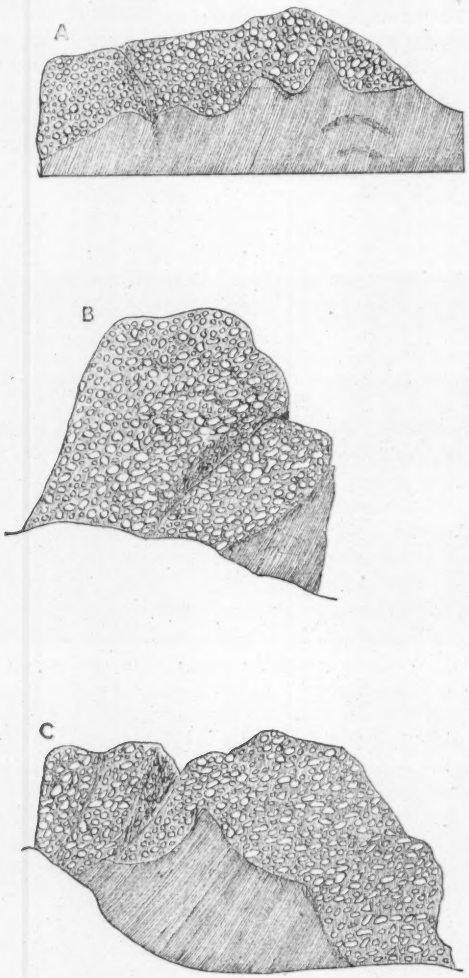



Fig. 7.—Sections on Y Bigl showing conglomerate lying on banded slate. A, B, C are in order from east to west.

memoir; but the dips Miss Raisin quotes have nothing to do with the matter. If my assailants will look at my former fig. 3 and my present Fig. 7 they will see that there are all varieties of dip to choose from, from 90° downwards, but then they have overlooked

my words, "eliminating contortions." If I spread out a fan on a table it may be said to lie horizontally upon it, yet all its parts will be highly inclined, and even the horizon itself on a stormy day at sea has not a single level spot upon it. In such a squeezed district as Y Bigl it is not by local dips in individual sections, but by the position of corresponding outcrops, that we must judge of the lie of the stratum as a whole.

*The Microscopic Evidence.*

But what does the microscopic evidence amount to? That some of the minuter fragments (for they require the microscope to see most of them) are derived from the same kind of rocks, whether they be found in Cambrian or Post-Llanberis strata. These rocks are for the most part of a common kind, with no particular character to render identity certain, but we will grant they are the same. At that early epoch there was not much choice of materials to derive a fragment from, and any of them must have come either from Cambrian rocks themselves or from the Pre-Cambrian of the neighbourhood. In the wearing down of the Cambrian strata to make the conglomerates, the elements of the coarser rocks in them would yield smaller elements to the succeeding rocks, and these would be found in the matrix. But the character of the later rock would be shown by the larger fragments it contains. The coarser Post-Llanberis rocks are always thus distinguishable. The rederivation of some parts of the conglomerates is interestingly shown at Moel Tryfaen, where one of the pebbles is itself conglomeratic.

It is not the fragments that are identical in the two series that present any difficulty, but rather the origin of those larger stones whose home we can find neither in Cambrian nor Pre-Cambrian rocks, but which closely resemble rocks of Post-Llanberis age. Some of these perhaps might be matched approximately in Anglesey, but they seem too large to have come so far. I think it not at all an unreasonable hope that we may some day find a fossiliferous pebble. I much regret that my opponents in this matter should have taken up the other side of the question, for, if they had sought to do so, there is no one more likely to be able to tell us whence the large pebbles may have come. In such an investigation the microscope would be doing its proper work; but it cannot prove by the similarity of small fragments the continuity of the deposits containing them, any more than it can disprove by the difference of the fragments the identity of two conglomerates, as has been already shown in the case of Twt Hill and Careg Goch. The worker with the microscope must not forget that in geology he is the servant of the stratigraphist, for until he knows the conditions of occurrence of his rocks his account of them has no *geological* value. It is therefore quite beyond his province to attempt to prove microscopically such a stratigraphical conclusion as the non-existence of an unconformity. The attempt in the present instance is founded on the assumption that rocks formed when "the same rocks were undergoing denudation" must belong to the same

series, and cannot be divided by an unconformity. That this assumption is unwarranted has again and again been proved. The break at the base of the Llandovery alluded to by our authors is, both in Shropshire and at Llandovery, between rocks of very similar character; and such is the case, it is generally stated, between the upper and lower parts of the Old Red Sandstone in Herefordshire, and between the Elgin and the Old Red Sandstone in Scotland. In most of these cases the separation is effected by the aid of the fossils, but in the present case stratigraphy has to do the work alone, and it is perfectly capable of doing it.

I trust that in the above remarks I may in no case have made a statement without at the same time indicating plainly how it may be checked, nor quoted my own opinion without giving any reasons for it, both of which procedures I hold to be inconsistent with scientific argument.

## NOTICES OF MEMOIRS.

I.—Herstellung von Diamanten in Silikaten, entsprechend dem natürlichen Vorkommen in Kapland. Vortrag gehalten im Verein zur Beförderung des Gewerbfleisses am 7 Februar, 1898, von I. Friedländer. (Berlin, 1898.)

(Artificial Production of Diamond in Silicates, corresponding to the Actual Mode of Occurrence in South Africa.)

IN the recent diamond-making experiments of M. Moissan, fused iron rich in carbon was allowed to cool in such a way that the separation of the excess of carbon took place under pressure, and it was thought that a high pressure was necessary to the success which had been attained. It is now known that the necessary pressure is not very high, for microscopic diamonds have been found as normal constituents of ordinary cast iron. In South Africa no iron is present in the metallic state in the diamond-bearing rock, although it is largely present as a chemical constituent of the stony matter. Hence, in regarding Moissan's method as being possibly identical with the one by which the South African diamonds had been formed, it was necessary to surmise that the crystals, after formation in the molten iron at some great depth below the earth's surface, had floated into the molten silicate-material above. It was, however, soon pointed out that the diamond-bearing rock, if in a state of fusion at small pressure, dissolves any diamonds contained in it.

Dr. Friedländer fused a small piece of olivine, a centimetre in diameter, by means of a gas-blowpipe, kept the upper portion in the molten state for some time by playing upon it with the flame, and stirred it with a little rod of graphite. After solidification the silicate was found to contain a vast number of microscopic crystals, but only in the part which had been in contact with the carbon. These Dr. Friedländer has subjected to a careful examination. They are octahedral or tetrahedral in form, are unattacked by hydrofluoric and sulphuric acids, have a high refractive index, sink slowly in

methylene-iodide, burn away when heated in a current of oxygen, and are unaltered if heated in a current of carbonic acid: the stony matter containing them scratches corundum. Hence Dr. Friedländer infers that they are diamond, and that the South African diamond may have been actually formed, as already suggested, by the action of a molten silicate, such as olivine, on graphite: carbonaceous shales are interrupted by the diamond-bearing rock, and numerous fragments of the shale, much altered, are found enclosed in the rock itself. The paper is illustrated with seven micro-photographs.

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II.—Note on the Occurrence of Diatomaceous Earth at the Warrumbungle Mountains, New South Wales. By Professor T. W. Edgeworth David, B.A., F.G.S. Proc. Linn. Soc. New South Wales, 1896, pt. 2, pp. 261–268, pls. xv–xvii.

Diatomaceous Earth Deposits of New South Wales. By G. W. Card, F.G.S., and W. S. Dun. Records Geol. Surv. New South Wales, vol. v, pt. 3, 1897, pp. 128–148, pls. xii–xv.

DEPOSITS of Diatomaceous Earth occur not infrequently in Victoria and Queensland as well as in New South Wales, but so far there is no record of them in South Australia or in Western Australia. They are widely distributed in the older colony of New South Wales, for deposits are known at Cooma, about 260 miles to the south of Sydney; at Bathurst and the Mundooran district, 250 miles to the west; and also in the Warrumbungle Mountains, the Richmond River district, and at Barraba, from 300 to 350 miles to the north of Sydney.

At Cooma the deposit is over 20 feet in thickness; it lies in a hollow partly inclosed by hills of basalt, and is now only covered by surface soil. In the Warrumbungle Mountains there is a bed of diatomaceous earth 3 ft. 9 in. in thickness, interstratified in trachytic rocks, which are regarded by Professor David as of early Eocene or late Cretaceous age. In the Richmond River district the earth is found in depressions of scoriaceous basalt, and is overlaid by beds of the same material. At Barraba the diatom bed is eight feet in thickness, with a single intermediate band of coarse sand two inches in thickness; beneath it are mudstones and lava fragments imbedded in diatomaceous material, and it is overlaid by a flow of basalt considered by Mr. E. F. Pittman to be of Miocene age. This covering of basalt is now the summit of an elevated tableland.

In these various deposits there is a very close resemblance in the character of the diatomaceous earth, which is a light, whitish, powdery material, typically similar to that known from other parts of the world. In some instances the siliceous constituents have been partially dissolved, and now form bands and nodular masses of hard, homogeneous, colloid silica. Chemical analyses show from 81 to 97 per cent. of silica, with, as a rule, small amounts of ferric oxide, alumina, and carbonates of lime and magnesia.

Microscopic examination shows, further, a very singular uniformity in the diatoms composing these different deposits in New South

Wales, for they consist almost exclusively of two or three varieties of the genus *Melosira*, and, rarely, a few examples of *Navicula*. With the diatoms there is also a small proportion of acerate spicules of fresh-water sponges. The markings of the diatoms are as perfectly preserved as in recent forms. Detrital materials are absent in the beds, but in a few instances impressions of leaves of plants have been noticed. All the deposits are distinctly of fresh-water origin, and probably of Tertiary age. Professor David considers that the constant association of volcanic rocks with the diatomaceous beds is not accidental, as probably hot springs, and the lavas also, furnished supplies of silica to the lakes in which the diatoms lived. It is evident that the preservation of these beds is in many instances due to being overlaid by basaltic and trachytic lavas.

The diatomaceous deposits in Queensland, like those of New South Wales, mainly consist of *Melosira*, but in those of Victoria the variety of diatoms is considerably greater, and fourteen genera have been enumerated in some of the fresh-water beds.

G. J. H.

## REVIEWS.

I.—Fossil Plants. For Students of Botany and Geology. By A. C. Seward, M.A., F.G.S. Vol. I. 8vo; pp. xviii, 452, with 111 illustrations. (Cambridge: Messrs. C. J. Clay & Sons, 1898. Price 12*s*.)

PALÆOBOTANY is no new creation of the *fin du siècle*, although, indeed, the subject has undergone considerable modification with the advance of botanical knowledge, more especially since it has been so largely assisted by the advances made in histological research.

We cannot but recall with gratitude the labours of such men as Sternberg, A. Brongniart, Lindley and Hutton, Göppert, Bowerbank, Schimper, Hooker, Williamson, Carruthers, De Saporta, Grand'Eury, and many others, who have paved the way for the botanist of to-day who desires to take up the study of Fossil Plants.

The author of the present volume is already favourably known as filling the office of Lecturer in Botany in the University of Cambridge, and has been a worker for the last ten years at palæobotany, one of his early papers having appeared in this Magazine for 1888 (p. 289); he is also the author of two volumes of a "Catalogue of Mesozoic Plants in the British Museum" (1894–5), and of several other papers of importance communicated to the Geological Society and elsewhere.

He tells us that "The subject of Palæobotany does not readily lend itself to adequate treatment in a work intended for both geological and botanical students. The botanist and geologist are not always acquainted with each other's subject in a sufficient degree to appreciate the significance of palæobotany in its several points of contact with geology and recent botany. . . . . It needs but

a slight acquaintance with geology," he thinks "for a botanist to estimate the value of the most important applications of palæobotany; on the other hand, the bearing of fossil plants on the problems of phylogeny and descent cannot be adequately understood without a fairly intimate knowledge of recent botany." "The student of elementary geology is not, as a rule, required to concern himself with vegetable palæontology, beyond a general acquaintance with such facts as are to be found in geological textbooks. The advanced student will necessarily find in these pages much with which he is already familiar; but this is to some extent unavoidable in a book which is written with the dual object of appealing to botanists and geologists."

While we cordially admit that "a fairly intimate knowledge of recent botany" is needful, if one proposes to take up such difficult questions as "the bearing of fossil plants on the problems of phylogeny and descent," yet, on the other hand, the recent botanist who takes up the study of fossil plants with only "a slight acquaintance with geology" is quite as likely to come to grief. Indeed, the botanist who ventures into the domain of palæobotany must be well-equipped with geological, mineralogical, and chemical knowledge, if he would attempt to interpret correctly the many structural and stratigraphical problems which lie before him at the very threshold of his investigation.

The first chapter is devoted to a historical sketch showing the dawn and development of geological ideas, more particularly those relating to fossil plant-remains; and the gradual evolution of accurate and intelligent observation which superseded the theorists of the last century. In the second the author treats of the relation of palæobotany to botany and geology. Here one naturally finds the methods of using fossils by the stratigraphical geologist, solely interested in determining the relative age of fossil-bearing rocks, contrasted with the intelligent study of fossils by the zoologist and the botanist, anxious to inquire into questions of biological interest which centre round the relics of ancient faunas and floras.

Alas! how few zoologists really interest themselves in fossil remains of extinct animals, seeing neither form nor comeliness, nor anything to desire in them! Indeed, our author admits that "the botanist, whose observations and researches have not extended beyond the limits of existing plants, sees in the vast majority of fossil forms merely imperfect specimens, which it is impossible to determine with any degree of scientific accuracy"! "He prefers to wait for perfect materials; or, in other words, he decides that fossils must be regarded as outside the range of taxonomic botany."

This has been really the attitude of both zoologists and botanists with regard to palæontology until within the last twenty years. Now all is changed, and the geologist is told by the biologist that in the future he need not concern himself with fossil organic remains; they will be taken over into abler hands than his own and properly dealt with. We are heartily glad to hear that this is to be so; but where, we venture to ask, would our science have been

during the past fifty years if we had not maintained a school of palæontologists, who have kept alive an interest in fossil remains, and who have been content to study and describe, to the best of their ability, even the imperfect specimens which were obtained, without waiting for the perfect materials which were not attainable?

Referring to the dual aspect of palæontology, as treated of by the biologist and the geologist, Mr. Seward appropriately quotes Humboldt, who fifty years ago wrote: "The analytical study of primitive animal and vegetable life has taken a double direction. The one is purely morphological, and embraces especially the natural history and physiology of organisms, filling up chasms in the series of still living species by the fossil structures of the primitive world. The second is more specially geognostic, considering fossil remains in their relations to the superposition and relative age of the sedimentary formations." To this we might now add that the one furnishes a guide to the past history and records the modifications which the *ancestors* of still living forms have undergone; the other shows us the life-range of each form in time and often its former distribution over the earth as well.

On the subject of "Fossil Plants and Distribution" we cordially agree with the author. "The present distribution of plants and animals represents one chapter in the history of life on the earth; and to understand or appreciate the facts which it records we have to look back through such pages as have been deciphered in the earlier chapters of the volume. . . . . In the case of particular genera the study of the distribution of the former species, both in time and space, that is geologically and geographically, points to rational explanations of, or gives added significance to, the facts of present-day distribution. That isolated conifer, *Ginkgo biloba*, L., now restricted to Japan and China, was in former times abundant in Europe and in other parts of the world. It is clearly an exceedingly ancient type, isolated not only in geographical distribution but in botanical affinities, which has reached the last stage in its natural life. The mammoth trees of California (*Sequoia sempervirens* and *S. gigantea*) afford other examples of a parallel case."[1]

The author takes us through a chapter on Geological History showing how strata have been built up; and then one on the preservation of plants as fossils. Both these subjects are carefully illustrated, and the student ought to be able to grasp some very good and clear notions on these subjects as he reads this part of the book.

A tougher chapter follows (v) "On the Difficulties and Sources of Error in the Determination of Fossil Plants," all of which is excellent reading; but the student working at the present day with the vast added lights invented for him in the past thirty or forty years, need not, like the Pharisee of old, thank God for his more exalted position to-day, nor forget entirely that but for the

[1] See the eloquent address by Professor Asa Gray to the American Association: Silliman's Amer. Journal, October, 1872, p. 282.

labours of past generations of workers, he and his Professors also might be priggishly discoursing with Dr. John Woodward on the exact time of year when the Deluge took place; or discover with Dr. Scheuchzer, of Zurich, in a giant fossil salamander from Oeningen, the image of "*Homo diluvii testis.*"

Under the subject of Nomenclature and the Rule of Priority, Mr. Seward writes: "Some writers would have us conform in all cases to this rule of priority, which they consistently adhere to apart from all considerations of convenience or long-established custom. . . . . A name may have been in use for, say, eighty years, and has become perfectly familiar as the recognized designation of a particular fossil; it is discovered, however, that an older name was proposed for the same species ninety years ago, and therefore, according to the priority rule, we must accustom ourselves to a new name in place of one which is *thoroughly established by long usage.*" In all this, and much more under this head, we are heartily in accord with the author, and would go so far as to follow the example of Pope Gregory in the "Ingoldsby Penance," and say—

> "Go fetch me a book! go fetch me a bell
> As big as a dustman's! and a candle as well:
> I'll send him—where, good manners won't let me tell!"

In Part II, Systematic, the author begins with the simplest type of very minute organisms, the THALLOPHYTA, as *Perediniales* (small single-celled organisms); he then treats of Coccospheres and Rhabdospheres. *Peredinium* was described by Ehrenberg in 1836 from Cretaceous beds in Saxony, while the others occur in the English Chalk and Lias, and all have been found nearly everywhere in sea-water. Other microscopic bodies are also noticed by the author, as the Chroococcaceæ, *Girvanella*, *Zonatrichites*; *Schizomycetes* (*Bacilli*); Algæ. Among these last, the student is warned against the many *spurious* fossil organisms pointed out by Williamson, Nathorst, and others, which simulate plant-remains. Then follow accounts of true fossil Algæ and of objects simulating Algæ. To these succeed recent and fossil Diatoms; Chlorophyceæ, Siphoneæ, and Confervoideæ; Rhodophyceæ; Phæophyceæ; Myxomycetes; Fungi. Most of these forms are much too obscure, and involve problems far too difficult for the elementary student to attack. In the Characeæ we begin to reach forms which even the young student of palæobotany may readily recognize and appreciate, occurring as they do in the Jurassic and Wealden, and in many of the Tertiaries of England and France.

Thence we pass on to the second half of the book, viz., the Mosses, the Equisetales or Equisetaceæ, with chapters on *Calamites* and *Sphenophyllum.* Here structures of stems, roots, foliage, the spore-cones and spores of these plants, give abundant subjects for illustration, and we should have been glad if more could have been given had space permitted. A list of authors and their works referred to in the text and an index usefully and appropriately end this volume.

As higher forms are reached no doubt the subject-matter will increase in interest. The author publishes in his preface the names of many people who have helped him in his work, but to none is he more indebted than to Mrs. Seward, "who has drawn by far the greater number of the illustrations," and we may add *very well done indeed*! In Volume II the author promises us that the systematic treatment of plants will be concluded, and the last chapters will be devoted to such subjects as geological floras, plants as rock-builders, fossil plants and evolution, and other general questions connected with palæobotany.

In conclusion, we cannot part from Mr. Seward without expressing our conviction that the volume before us will prove a most acceptable addition to the Biological Series of Cambridge Natural Science Manuals issued by Messrs. C. J. Clay & Sons from the University Press.

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II.—A New Geological Map of England and Wales, reduced from the Ordnance and Geological Surveys, and published with Government Authority under the direction of Sir Archibald Geikie, D.C.L., LL.D., F.R.S. With Descriptive Text. By John Bartholomew & Co. Mounted on cloth and in case, 12*s.* 6*d.*; on cloth with rollers and varnished, 17*s.* 6*d.* (Edinburgh, 1897.)

THIS map is on a scale of an inch to ten miles, and is slightly larger than Ramsay's hand-coloured map published by Stanford. The colour-printing adopted by Bartholomew is excellent, but the tints are not quite so effective as those laid down by hand on the older map, and this is noticeable more especially as regards the Cretaceous and Tertiary strata. The colours, however, are clear and transparent, the topography is accurate, and the railways are shown up to date, including nearly the whole of the Great Central Railway, which is not yet open to the public. Those parts of Scotland, Ireland, and France which are included in the map are coloured geologically, and there are four longitudinal sections to depict the structure of the country from Holyhead to Beachy Head, from Denbigh to Saltfleet, from the Solway Firth to Flamborough Head, and across the Isle of Wight. The map is also accompanied by 28 pages of explanatory notes, which give a concise account of the strata. Future work will no doubt introduce some detail into the large area of "Lower Silurian" rocks in Central and South Wales, and also into the Devonian rocks. We note that the Plymouth limestone is coloured blue, but the Torquay and Brixham limestones are not so distinguished. We observe that the colour adopted for Triassic marls is not at first sight readily to be distinguished from that of the Permian marls and sandstones; but there is much yet to be done in discriminating Permian and Trias in the field in the Midland and Northern Counties. The Rhætic beds are indicated in places where they have been mapped by the Geological Survey, but, as

remarked in the text, "this group of strata runs with singular persistence throughout England and Wales."

The price of this map is 12*s.* 6*d.*, and as it is unquestionably the best geological map of England and Wales which can be conveniently carried in the pocket, it should meet with a cordial welcome from all interested in the physical structure of our country.

## REPORTS AND PROCEEDINGS.

### Geological Society of London.

I.—March 9, 1898.—W. Whitaker, B.A., F.R.S., President, in the Chair.

Professor J. W. Judd exhibited, on behalf of the Coral Reef Committee of the Royal Society, the lowest core (698 feet) from the boring at Funafuti (Ellice Islands), and drew attention to the remarkable changes exhibited by the rocks obtained at this depth. The core from this boring (a mass of material more than a ton in weight) had been sent to this country by Professor Edgeworth David, and was now being submitted to careful study. The last 20 or 30 feet of the boring was carried on in a rock which was of a very soft character, and highly but minutely crystalline. Microscopic examination shows that the rock is almost completely converted into a mass of very small rhombohedra, the organic structures being nearly obliterated; while a preliminary chemical examination seems to indicate that magnesia has been introduced into the rock to a considerable extent. The complete study, microscopical and chemical, of all the stages of the change which has taken place in this rock—a study which will be undertaken by Mr. C. G. Cullis—promises to throw much light on processes of rock-formation of very great interest to the geologist.

The following communications were read:—

1. "Note on Clipperton Atoll." By Rear-Admiral Sir W. J. Wharton, K.C.B., F.R.S., Hydrographer to the Admiralty. (Communicated by Sir Archibald Geikie, D.Sc., F.R.S., F.G.S.)

This atoll, 600 miles from North America, in lat. 10° 17′ N., long. 109° 13′ W., possesses a lagoon which is now completely cut off from the sea. In this is a perfectly round hole where soundings of 20 fathoms or more are reported, on the authority of Mr. Arundel, and even deeper ones on that of the captain of a merchant-vessel. On the coral ring there rises a mass of modified trachyte, the subject of the following communication, about 60 feet in height. The great depth of the lagoon and the rock-mass on the ring are not compatible with the origin of the reef by subsidence or outward growth; and the possible hypothesis is put forth that this reef had grown on the lip of a volcanic crater, or on an island, such as Krakatao, in which the interior has been enlarged and deepened by volcanic explosion.

2. "A Phosphatized Trachyte from Clipperton Atoll." By J. J. H. Teall, Esq., M.A., F.R.S., V.P.G.S.

Specimens from the projecting rock described in the preceding communication are dark brown, white, or cream-coloured. The brown specimens are trachytes, composed of glassy phenocrysts of sanidine set in a groundmass of microlitic felspars with brown interstitial matter. The light-coloured rocks are more or less altered trachytes, in some of which the glassy phenocrysts of sanidine may still be recognized. Analyses of several specimens show that the rocks all contain varying amounts of phosphoric acid, as indicated by the following table :—

| | I. per cent. | II. per cent. | III. per cent. |
|---|---|---|---|
| $SiO_2$ ... ... ... | 54·0 | 43·7 | 2·8 |
| $P_2O_5$ ... ... ... | 8·4 | 17·0 | 38·5 |
| Loss on ignition ... | 3·1 | 12·3 | 23·0 |

The last specimen consists of 95 per cent. of hydrated phosphate of alumina, with some iron, having thus a composition allied to the so-called redonite from Redonda in the West Indies. The progressive alteration affects first the groundmass, then the microlitic felspars, and lastly the porphyritic crystals of sanidine; and it is probable that the change has been effected by solutions of alkaline phosphate and other compounds derived from the droppings of sea-birds. A somewhat similar phosphate, shipped from Connétable Island off French Guiana, is referred to on the authority of Mr. Player.

3. "The Pliocene Deposits of the East of England.—Part I. The Lenham Beds and the Coralline Crag." By F. W. Harmer, Esq., F.G.S.

From the discussion of lists of fossils, a large number of sections, and a series of borings, the author endeavours to establish the following propositions :—

I. With regard to the Lenham Beds :

(*a*) That they are older than the Coralline Crag, thirteen out of sixty-seven mollusca found in them being characteristic Miocene or Italian Lower Pliocene forms unknown or very rare in the latter formation.

(*b*) These beds had probably been upheaved, consolidated, and exposed to denudation before the deposition of the Coralline Crag, and may have been, as formerly suggested by Professor E. Ray Lankester, the source from which the "boxstones" found at the base of the Suffolk Crag have been derived. These boxstones contain a fauna, not identical with, but of the same general character as that of Lenham.

(*c*) In the interval between the deposition of the Lenham Beds and the Coralline Crag the sea retired to the north, in consequence of the upheaval of the southern part of the area, as it did in Belgium towards the close of the Diestien period.

(*d*) The Lenham Beds are most nearly, though not exactly, represented by the Zone à *Terebratula grandis* of Belgium, and

possibly by some fossiliferous deposits recently discovered at Waenrode, near Diest, the Coralline Crag corresponding very closely with the Belgian Zone à *Isocardia cor.*

II. With regard to the Coralline Crag:

(*a*) That the junction of the Crag with the London Clay dips to the N.N.E.

(*b*) That no satisfactory evidence, either stratigraphical or palæontological, is forthcoming to show that any divisions to be observed in this formation at Sutton are persistent at other localities, and that species which have been tabulated as characteristic of certain horizons are found also in other parts of the Coralline Crag, and often in the Red Crag as well.

(*c*) That there is no evidence of any great subsidence, of deep-sea conditions, of great changes of climate, or of the operation of floating ice during the period. The climate was warmer than that of Britain at the present day, more nearly approaching that of the Mediterranean or the Azores.

(*d*) That, so far from it being possible to separate this Crag into eight zones, the twofold division hitherto adopted, into shelly incoherent sands and indurated rock, can no longer be maintained, the latter being merely an altered condition of the former, as proved by the discovery of a section showing the two types passing laterally into each other.

(*e*) That, with the exception of the base, this Crag forms a consistent and continuous whole, accumulated under similar conditions, namely, in the form of submarine banks, piled up by currents in sheltered situations like that known as the Turbot Bank off the Antrim Coast and those at the south of the Isle of Man, where Professor Herdman's "neritic" deposits occur.

(*f*) That the German Ocean was less open to the north during the Coralline Crag period than at present, but that it was connected with the Atlantic by a channel over some part of the southern counties of England.

III. With regard to the Red Crag:

That it forms, with the exception of the Chillesford Beds and "the unfossiliferous sands of the Crag," a continuous sequence of deposits arranged horizontally, and not vertically. It was a marginal accumulation of a sea slowly retreating to the north and east, as shown by the gradually increasing number of northern mollusca met with in this direction.

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II.—March 23, 1898.—W. Whitaker, B.A., F.R.S., President, in the Chair. The following communications were read:—

1. "The Eocene Deposits of Devon." By Clement Reid, Esq., F.L.S., F.G.S. (Communicated by permission of the Director-General of H.M. Geological Survey.)

A re-examination of the area around Bovey has led the author to think that Mr. Starkie Gardner is probably right in referring the

supposed Miocene strata to the Bagshot period. Lithologically, as well as botanically, the deposits in Devon and Dorset agree closely. The gravelly deposits beneath the Bovey pipeclays are also shown to belong to the same period, and not to be of Cretaceous date. This correction has already been applied by Mr. H. B. Woodward to a large part of the area. The plateau gravels capping Haldon are also considered to belong to the Bagshot period, for they correspond closely with the Bagshot gravels of Dorset to the east, and of the Bovey Basin to the west, and possess peculiarities which distinguish them from any Pleistocene Drift.

2. "On an Outlier of Cenomanian and Turonian near Honiton, with a Note on *Holaster altus*, Ag." By A. J. Jukes-Browne, Esq., B.A., F.G.S. (Communicated by permission of the Director-General of H.M. Geological Survey.)

Although an outlying patch of Chalk in the parish of Widworthy was mentioned by Fitton and marked on De la Beche's map, it has not yet been described. The tract is about $4\frac{1}{2}$ miles south-west of Membury, $3\frac{1}{2}$ miles east of Honiton, and about 7 miles from the coast at Beer Head.

The quarries at Sutton are almost entirely obscured by vegetation, but the following approximate section was obtained from a mason who formerly worked in them :—

| | | feet. |
|---|---|---|
| | 7. Flint-rubble ... ... ... ... ... | 4 to 6 |
| [Zone of *T. gracilis.*] | 6. Soft white Chalk ... ... ... ... | 10 to 30 |
| [Zone of *Rh. Cuvieri.*] | 5. Hard Chalk ... ... ... ... ... | About 20 |
| | 4. Freestone ... ... ... ... ... | ,, 5 |
| | 3. Soft Chalk with green grains ... ... | ,, 5 |
| | 2. Hard cockly Chalk ... ... ... ... | ,, 2 |
| | 1. "Grizzle" (a hard calcareous sandstone). | |

The Freestone, used locally for building, is evidently identical with the Beer Stone.

Another small outlier of Turonian Chalk occurs at Wilmington, resting on hard quartziferous limestone with glauconitic grains, which yielded fossils indicating its equivalence with the uppermost Cenomanian beds of the coast-section. Below this come other sandstones, sometimes containing lumps of "grizzle," giving a total thickness of 40 or 42 feet to these beds on the whole—a much greater thickness than is ever attained on the coast. A list of fossils is appended to the paper, and the author discusses the affinities of *Holaster altus*, throwing out the suggestion that there is a gradation from *H. Bischoffi* through *H. altus* to *H. subglobosus*.

3. "Cone-in-Cone: Additional Facts from Various Countries." By W. S. Gresley, Esq., F.G.S.

Examples of flinty stone in the "fire-clay series" of the Ashby coalfield exhibit "areas of conic structure, lying unconformably." In the same stratum of shale are large masses of the same flinty rock, more or less coated with conic structures, which appear to have been formed out of layers of shale and ironstone. The

bending-up of the shale above the nodules and down below them, the close but unconformable covering of Permian breccia, and the staining of the whole section suggests, if indeed it does not demonstrate, to the author that the growth of the cone-in-cone took place subsequently to the deposit of the Permian breccia. Several American and other examples are described, and a series of conclusions are appended to the paper.

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III.—April 6, 1898.—W. Whitaker, B.A., F.R.S., President, in the Chair.

Professor T. Rupert Jones exhibited and commented upon a series of large stone implements, sent to England by Mr. Sidney Ryan, from the tin-bearing gravels of the Embabaan in Swaziland (South Africa). They consist of fine-grained quartzite, chert, lydite, siliceous schist, and quartzites composed of breccia and grit-stones; one of the latter mylonized. He also exhibited some corresponding rock-specimens from the neighbouring Ingewenyaberg, with a map and section prepared by Mr. S. Ryan. Some similar implements from the same district, lent by Mr. Nicol Brown, F.G.S., and some analogous implements of rough quartzite, from Somaliland, lent by the Rev. R. A. Bullen, F.G.S., were also exhibited.

Professor H. G. Seeley exhibited the humerus of a Plesiosaurian in which the substance of the bone was almost entirely replaced by opal. He explained that the fossil was from the opal-mines of New South Wales. Externally there is no indication of its internal condition as a pseudomorph, and it had been broken to ascertain its commercial value as opal. It is translucent; of a bluish tint, with a slight red fire. So far as he was aware, it was the only example of a fossil bone in this condition; and he was indebted to Messrs. Hasluck, the opal merchants, for the opportunity of placing the specimen before the Fellows.

The following communications were read:—

1. "On some Palæolithic Implements from the Plateau-Gravels, and their Evidence concerning 'Eolithic' Man." By W. Cunnington, Esq., F.G.S.

Although at first inclined to believe that the chipping on the "Eoliths" of the plateau-gravels was the work of man, the author has been led to recant this opinion by the detailed study of specimens lent or given to him by Mr. B. Harrison. His reasons are mainly based on the facts that the chipping is of different dates, even upon the same specimen, and that it was produced after the specimens were embedded in the gravel.

A further series of specimens, which, although not found actually *in sitú* in the gravels, present undoubted evidence that they came from these, are considered by the author to be of Palæolithic type. One of them appeared to have gone through the following stages:—first it was fashioned by man into a Palæolithic implement; then it

was abraded, broken, and chipped along one edge in the same fashion as the alleged "Eolithic" working; finally it was stained, marked with glacial striæ, and covered with a thin layer of white silica.

This implement appears to prove that Palæolithic man lived on the Kentish plateau before or during the deposit of the plateau-gravels, and that the "Eolithic" chipping is not the work of man.

2. "On the Grouping of some Divisions of Jurassic Time." By S. S. Buckman, Esq., F.G.S.

The author argues for an arrangement in the division of Jurassic time based upon the zoological phenomena of the Ammonite-fauna. He considers that such time-divisions should be related to the duration of Ammonite families. He divides the Jurassic Period into two epochs—the Eojurassic and the Neojurassic: the former the time when the Ammonite families of the Arietidæ and their close ally the Hildoceratidæ were dominant; the latter commencing just upon the extinction of these families, and being the time when the Stepheoceratidæ held chief sway.

The epochs are subdivided into ages, and the ages, again, are divided into hemeræ—a hemera being the chronological unit. Reasons are given for the different subdivisions, and for commencing the Eojurassic Period with the *rotiformis*-hemera.

The Eojurassic Period it is proposed to divide into four ages—the Sinemurian, the Pliensbachian, the Toarcian, and the Aalenian.

During the Sinemurian age, whereof the zoological phenomenon is the acme and paracme of the Arietidæ, was deposited a part of the Lower Lias, beginning with the zone of *Ammonites Bucklandi* and ending with that of *A. oxynotus*. This age is divided into the following seven hemeræ, stated in ascending order: *rotiformis, Gmuendensis, Birchi, Turneri, obtusi, stellaris, oxynoti.*

During the Pliensbachian age, marked by the dominance of Deroceratidæ and Amaltheidæ, was laid down the rest of the Lower and almost all the Middle Lias. It includes seven hemeræ, namely: *raricostati, armati, Jamesoni, Valdani, striati, margaritati, spinati.*

During the Toarcian age, when the *Dumortieriæ* and a part of the Hildoceratidæ were prominent, the following strata accumulated: a small part of the Middle and the whole of the Upper Lias, the Cotteswold Sands, the Midford Sands, and a portion of the Yeovil Sands. There are ten hemeræ: *acuti, falciferi, bifrontis, Lilliæ, variabilis, striatuli, Struckmanni, dispansi, Dumortieriæ, Moorei.*

During the Aalenian age, when there was a preponderance of another portion of the Hildoceratidæ which may be known as the *Ludwigia*-group, and of *Hammatoceras*, the rest of the Yeovil Sands and a part of the Inferior Oolite were the accumulated deposits. This age is divided into the following six hemeræ: *Aalensis, opaliniformis, scissi, Murchisonæ, Bradfordensis, concavi.*

Part of the Neojurassic division is separated into two ages. During the first, the zoological phenomenon is the acme and

paracme of *Sonninin*æ; during the second, the predominance of *Parkinsoni*æ.

The paper contains a hemeral timetable of the Eojurassic Period and part of the Neojurassic, a genealogical table of Ammonite development during the same and a previous portion of time, notes on certain generic names, and a list of the Ammonite genera referred to.

## CORRESPONDENCE.

### A MECHANICAL THEORY OF THE DIVINING-ROD.

The review in *Nature* (1897, pp. 568, 569) of a publication relating to the "divining-rod," recalls to my mind a purely mechanical theory of that rod, which was given me years ago by a friend.

This theory has been repeatedly tested by me and shown to be correct in the presence of my classes. The process is exceedingly simple. Take any forked twig of a reasonably tough fibre in the clenched hands with the palms upward. The ends of the limbs forming the twig fork should enter the closed fists on the exterior side of each fist, i.e. on the two sides of the clenched hands furthest from each other.

When a twig is grasped in this position it will remain stationary if held loosely, or with only a moderately firm grasp; but the moment the grasp is tightened, the pressure on the branches will force the end of the twig to bend downwards. The harder the grip the more it must curve.

The curvature of the twig is mechanically caused by the pressure of the hands forcing the limbs to assume a bent and twisted position; or the force that causes the forked limb to turn downwards is furnished by muscles of the hands, and not from any other cause.

The whole secret of the "divining-rod" seems to reside in its position in the hands of the operator, and in his voluntarily or involuntarily increasing the closeness of his grasp on the two ends of the branches forming the fork.

If the above conditions are fulfilled the twig will always bend downwards—water or no water, mineral or no mineral; anyone can be an operator, and any material can be used for the instrument, provided the limbs forming the fork are sufficiently tough and flexible.

It can be easily understood how an ignorant operator may deceive himself, and be perfectly honest in supposing that some occult force, and not his hands, causes the fork to curve downwards.

M. E. Wadsworth.

*Michigan College of Mines, Houghton, Michigan.*

### *PLACOPARIA* FROM THE SKIDDAW SLATES.

Sir,—As supplementary to the note published by Miss G. L. Elles in the March number of this Magazine (p. 141) recording the occurrence of *Placoparia* in the Skiddaw Slates, it may be of interest to mention that one of the three specimens in the Woodwardian Museum was correctly named and labelled as long ago as the year 1890, when it was collected by Mr. H. Kynaston, M.A., F.G.S., at Outerside during Professor Hughes' geological excursion to the Lake District. The second specimen, which was obtained at the same time and locality, was identified by me in 1895, when I was rearranging the collection, and was duly entered with the other in my manuscript catalogue of the fossils of the Skiddaw Slates in the Woodwardian Museum. The third specimen, as Miss Elles has mentioned, comes from Ellergill, and is a recent gift from Professor H. A. Nicholson, F.R.S.

F. R. Cowper Reed.

Woodwardian Museum, Cambridge.
*April*, 1898.

## MISCELLANEOUS.

New Geological Survey Maps.—In our January number, p. 48, attention was drawn to the issue of several sheets of the General Geological Map of England and Wales (scale an inch to four miles), published by the Geological Survey. The fifteen sheets of the colour-printed edition have now all been issued; and (with the exception of the title-sheet, price 2*s.*) the price of each sheet is 2*s.* 6*d.* The total cost of the map is therefore £1 17*s.* It is to be hoped that some of the one-inch Geological Survey maps, such as that of "London and its Environs," which in the hand-coloured form costs no less than 30*s.*, or that of the Isle of Wight, price 8*s.* 6*d.*, may ere long be issued in the cheaper form.

Geological Survey.—The vacancy caused by the retirement of Mr. George Sharman, senior Palæontologist on the Geological Survey, has been filled by the appointment of Mr. F. L. Kitchin, M.A., Ph.D., as Assistant Palæontologist, under Mr. E. T. Newton, F.R.S., Palæontologist.

A Liner in a Duststorm.—The Castle Line mail steamer "Roslin Castle" arrived at Plymouth on February 22 more than two days later than usual, and Captain Travers reported an extraordinary experience. He stated that on Monday, February 14, the vessel met what appeared to be a dense fog, but it proved to be a sandstorm, the air being permeated with red sand from the Sahara Desert for over 900 miles. During this time the sun and stars were obscured, and no observations were possible until after Madeira was reached.—*Daily Mail.*

1a

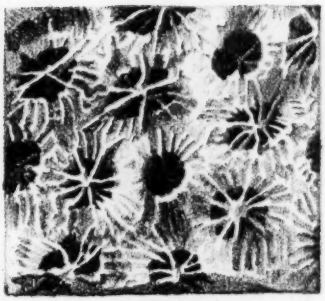
1b

2a

2b

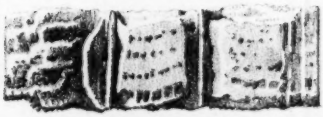
3a

3b



4a

4b × 2



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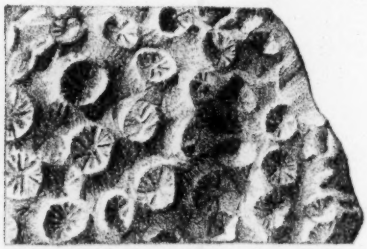

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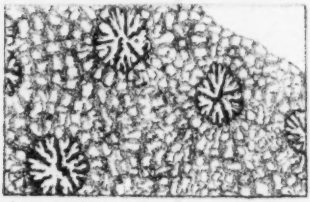
6 × 3



5b × 4

7 × 2

E. Drake del. et lith. West, Newman imp.

Egyptian Corals.